
CHERCHE LA VÉRITÉ

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY,

CONTAINING

PAPERS,

ABSTRACTS OF PAPERS,

AND

REPORTS OF THE PROCEEDINGS

OF

THE SOCIETY,

FROM NOVEMBER 1873 *TO JUNE* 1874.

VOL. XXXIV.

BEING THE ANNUAL HALF-VOLUME OF THE MEMOIRS AND PROCEEDINGS OF THE ROYAL ASTRONOMICAL SOCIETY.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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Professor Cayley, F.R.S., President, in the Chair.

J. Bridson, Esq., Bolton;
T. W. Bush, Esq., Cromwell Street, Nottingham;
W. H. Finlay, Esq., Royal Observatory, C.G.H.; and
Lieut. G. F. Guyon, St. Leonards-on-Sea,
were balloted for and duly elected Fellows of the Society.

The following Note is inserted by special order of the *Council*:—

The attention of the Council has been directed to certain remarks made by their late Editor, in paragraph 2, page 583, of the Supplementary Number, Vol. xxxiii. of the *Monthly Notices*. The Council have entered on their minutes a resolution expressing their strong disapprobation of the paragraph referred to. ?! ?!

On the rejection, in the Lunar Theory, of the term of Longitude depending for argument on eight times the mean longitude of Venus minus thirteen times the mean longitude of the Earth, introduced by Professor Hansen; and on the effect of that rejection upon the state of the Lunar Tables, and upon the lunar calculations which serve as basis for Ancient Chronology.

By George Biddell Airy, K.C.B., Astronomer Royal.

In a note, dated 1827, December 16, to a paper published in the *Philosophical Transactions* for 1828, I announced that I had discovered equations of long period in the movements of the

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Earth and *Venus* depending on the argument eight times the mean longitude of *Venus minus* thirteen times the mean longitude of the Earth, of which the period is 239 years; and I gave approximate values of the co-efficients. In a paper, dated 1831, November 8, published in the *Philosophical Transactions* for 1832, I gave the details of the computations. The whole process has been reverified, first by M. le Comte G. de Pontécoulant, secondly by M. Le Verrier, and is found to be correct.

From the first announcement of the discovery of this planetary equation, mathematicians, who were more specially engaged on the lunar theory, considered it probable that there would be found an inequality in the geocentric motion of the Moon depending on the same argument. In the summer of 1829, I visited M. Carlini, in the neighbourhood of Milan, and he immediately made inquiries of me as to the possible existence of such an inequality in the lunar motion. In a paper by M. Poisson, "sur le mouvement de la lune autour de la terre," dated 1833, June 17, and published in the *Mémoires de l'Académie Royale des Sciences de l'Institut de France*, tome xiii. 1835, there was given a calculation of the lunar inequality, which led M. Poisson to the conclusion that the inequality in the Moon's motion is insensible. It was, however, subsequently pointed out by M. Delaunay that Poisson's calculation was based only on the disturbance of the excentricity of the Earth's orbit, whereas the effect of the disturbance of the major axis of the Earth's orbit was more important. In this state the question rested for many years.

In 1847, Professor Hansen announced to the French Academy of Sciences (*Comptes Rendus*, tome xxiv. page 795), that he had discovered two equations of long period in the Moon's motion, depending respectively on the arguments $18 \times$ mean long. *Venus* $- 16 \times$ mean long. Earth $-$ Moon's mean anomaly, and $8 \times$ mean long. *Venus* $- 13 \times$ mean long. Earth, with periods respectively of 273 and 239 years. At the same time, Professor Hansen communicated to the Royal Astronomical Society a statement of the same general character, of which an extract is published in the *Monthly Notices* of the Society for 1847, May 14. Subsequent investigations (of which, I believe, no detailed accounts are published) induced him to make some alterations in the coefficients first adopted; and, finally, in the Lunar Tables, Professor Hansen adopted the following terms:—

$$+15''\cdot34 \times \sin\{18.\ \text{mean long. } Venus - 16.\ \text{mean long. Earth} - \text{Moon's mean anomaly} + 30^\circ\ 12'\}$$
$$+21''\cdot47 \times \sin\{8.\ \text{mean long. } Venus - 13.\ \text{mean long. Earth} + 274^\circ\ 14'\}.$$

The tables, affected with these equations, have been used by myself in the computations of the Eclipses of Thales, Larissa, Agathocles, and Sticklastad, *Memoirs of the Royal Astronomical Society*, vol. xxvi. page 131, &c.

The first of these equations has been recalculated by M. De-

launay. After a first failure (*Comptes Rendus*, tome xlix. page 293), the examination was repeated, and every step of the investigation is printed with the utmost detail, in the *Additions* to the *Connaissance des Temps*, 1862. M. Delaunay's result agrees substantially with Professor Hansen's. It is unnecessary, therefore, to allude further to this inequality.*

The second equation, also, depending on 8 × mean long. *Venus* − 13 × mean long. Earth, was recalculated by M. Delaunay, but with very different result. His investigations, which are given in detail in the *Additions* to the *Connaissance des Temps* for 1863, lead to the conclusion that the direct effect of *Venus* upon the Moon produces an equation of the form above-mentioned with coefficient $0''\cdot0039$; and that the indirect effect of *Venus*, by altering the Earth's distance from the Sun and its heliocentric longitude (especially the distance), and thus modifying the action of the Sun upon the Moon, produces an equation with coefficient $0''\cdot2723$. I am informed, in private communication from Professor Newcomb, that he has repeated the investigations with nearly the same result. These coefficients, in comparison with that found by Professor Hansen, may be described as insignificant.

Assuming then that the equation

$$+21''\cdot47 \times \sin\{8.\ \text{mean long. } \textit{Venus} - 13.\ \text{mean long. Earth} + 274^\circ\ 14'\}$$

ought to be withdrawn from Hansen's tables, it becomes matter of interest to ascertain what will be the effect of that withdrawal upon the Lunar Tables and upon the theory embodied in those tables. For this purpose, I must refer to my paper in vol. xxix. of the Society's *Memoirs*, "Corrections of the Elements of the Moon's Orbit, &c.," and specially to the section, beginning on page 9, and headed, "Values of R. and Correction of Moon's Epoch of Longitude and Mean Motion." It must be remembered that the Moon's observed longitudes have been compared with longitudes computed, principally by use of Damoiseau's Tables, with Damoiseau's Mean Longitudes, on Plana's Theory; that the results of the comparisons have been collected in groups of years; and that for each group the corrections of certain elements are computed. The only element which concerns us here is the Moon's Mean Longitude. The correction which must be applied

* I cannot dismiss this remarkable equation without expressing my opinion that its discovery is one of the greatest steps—perhaps the greatest step—in Gravitational Astronomy. My experience in the calculation of the planetary equation depending on {8. mean long. *Venus* − 13. mean long. Earth} enables me to appreciate the labour of Professor Hansen's calculation. The research by which it was suggested must also have been laborious. It may be interesting to the student of these investigations to remark that the final value of the coefficient depends almost entirely on the inclination of the orbit of *Venus* to the ecliptic. The relative position of the perihelia of *Venus* and the Earth, and the smallness of the excentricities, greatly diminish the terms independent of inclination. I had pointed out this circumstance in the planetary equation to which I have alluded.

to Damoiseau's Tabular Mean Longitude, in order to produce the Mean Longitude deduced from observation, is called R.

Now this comparison with Plana's Theory and Damoiseau's Mean Longitudes was made before the suggestion by Hansen of the two equations of long period produced by the action of *Venus*. Regarding the results of the means of theoretical terms of short period through a few years, as being essentially the same in the two theories, the mean longitude being always subject to correction, we could then add the numerical values of Hansen's two equations to Plana's Theoretical Longitudes, or we could subtract the values of the two equations from the values of R, in order to ascertain the true correction to Damoiseau's Mean Longitudes, which were used in the comparison of observation with theory. This process was used in the paper to which I have referred, and a series of values of "R − Hansen's Inequalities" is formed on page 9 of that paper, which it is not necessary to reprint here. It is sufficient to state that it was found on page 10, that the errors of the groups might be expressed in the mass by the formula $-2''\cdot48+2''\cdot771\times$ No. of group, measured from the middle group, and that, on substituting in this formula, the residual errors for the individual groups were not unreasonably large.

But the rejection of one of Hansen's equations invalidates the latter part of this process. It is necessary now to use only the inequality depending on 18 × mean long. *Venus* − 16 × mean long. Earth − Moon's mean anomaly. Mr. Christie has kindly computed for me the value of this equation for the middle of each group; and the determination of the correction to the Tabular Mean Longitude now stands as follows:—

Range of years in each group.	Value of R given by the equations.	Hansen's inequality of long period.	R—Hansen's inequality.
	"	"	"
1750 to 1759	− 3·19	+ 6·55	− 9·74
1755 to 1764	− 1·38	+ 4·91	− 6·29
1760 to 1768	− 0·31	+ 3·38	− 3·69
1765 to 1773	+ 1·43	+ 1·64	− 0·21
1769 to 1778	+ 3·04	+ 0·06	+ 2·98
1774 to 1782	+ 3·40	− 1·53	+ 4·93
1779 to 1787	+ 3·04	− 3·27	+ 6·31
1783 to 1791	+ 3·76	− 4·63	+ 8·39
1788 to 1796	+ 4·48	− 6·28	+ 10·76
1792 to 1801	+ 2·76	− 7·69	+ 10·45
1797 to 1805	+ 0·87	− 9·02	+ 9·89
1802 to 1810	+ 1·13	− 10·38	+ 11·51
1806 to 1815	+ 0·93	− 11·49	+ 12·42
1811 to 1819	− 0·61	− 12·49	+ 11·88
1816 to 1824	− 1·39	− 13·42	+ 12·03
1820 to 1829	− 1·11	− 14·13	+ 13·02
1825 to 1833	− 1·15	− 14·66	+ 13·51
1830 to 1838	− 1·58	− 15·08	+ 13·50
1834 to 1842	− 1·01	− 15·26	+ 14·25
1839 to 1847	+ 0·15	− 15·31	+ 15·46
1843 to 1851	+ 1·87	− 15·21	+ 17·08

The values in the last column may be best represented in a simple formula (as before), expressing the correction applicable to mean longitude and mean motion, by the expression $+ 8''.02 + 1''.097 \times$ No. of group, measured from the middle group. The following table exhibits the result of substitution in this formula :—

No. of Group.	Formula	Excess of R—Hansen's Inequality.	No. of Group.	Formula	Excess of R—Hansen's Inequality.	No. of Group.	Formula.	Excess of R—Hansen's Inequality.
	$''$	$''$		$''$	$''$		$''$	$''$
−10	−2·95	−6·79	−3	+4·73	+3·66	+4	+12·41	−0·38
−9	−1·85	−4·44	−2	+5·83	+4·93	+5	+13·51	−0·49
−8	−0·76	−2·94	−1	+6·92	+3·53	+6	+14·60	−1·09
−7	+0·34	−0·55	0	+8·02	+1·87	+7	+15·70	−2·20
−6	+1·44	+1·54	+1	+9·12	+2·39	+8	+16·80	−2·55
−5	+2·54	+2·39	+3	+10·21	+2·21	+10	+17·89	−2·43
−4	+3·63	+2·68	+2	+11·31	+0·57	+11	+18·99	−1·91

It appears, therefore, that after applying the best correction that we are able to give to the assumed mean longitude and mean motion, there still remains a series of unexplained discordances, varying from $-6''.79$ to $+4''.93$. And each of the discordances here exhibited is based upon about 440 observations; these observations are all reduced by the same sytem of clock-stars and other elements (Bessel's in the *Tabulæ Regiomontanæ*); and the tabular places are all computed by the same theory (Plana's, with Damoiseau's epochs).

I conceive it to be totally impossible that a complete and correct theory can leave such large and systematic discordances. And I express my opinion that there is still some serious defect in the Lunar Theory.

Supposing the formula above given to be accepted as a true exhibition of the correction required to Damoiseau's epochs of longitude, it would appear that (remarking that the mean interval of groups is 4·65 years) Damoiseau's annual motion ought to be increased by $0''.236$, or his secular motion ought to be increased by $23''.6$. In the former investigation (*Memoirs*, vol. xxix., page 10), Damoiseau's secular motion was increased by $59''.6$; and, as I understand, Hansen's secular motion was adopted from Damoiseau's, thus corrected. It would appear, therefore, that (still on the supposition that the formula above-mentioned is accepted) the secular motion of Hansen's Tables ought to be diminished by $36''$. We have now to examine the effects of this diminution on the eclipses of antiquity, which have been computed with Hansen's Tables.

The term which has been removed from the Tables vanishes about the year 1827. The eclipse of Agathocles occurred in the year −309; that of Larissa (as I accept it) in −556; that of Thales in −584. The intervals backwards, through which the altered

motion in longitude applies, are, therefore, 2136, 2383, and 2411 years respectively. The alterations of Moon's motion in longitude (adopting the formula of the last paragraphs) are 12′ 49″, 14′ 18″, 14′ 28″. The tabular places of the Moon will be increased by these quantities.

In my paper 'On the Eclipse of Agathocles, the Eclipse at Larissa, and the Eclipse of Thales,' published in the Society's *Memoirs*, vol. xxvi., page 147, I have remarked that the diminution of Secular Acceleration, from the value employed by Professor Hansen to that proposed by Professor Adams, and supported by the investigations of Delaunay and others, would diminish the tabular longitude of the Moon by 160″ for 1000 years. The effect for the intervals of years above given (proportional to the square of the time) would be 12′ 10″, 15′ 8″, and 15′ 30″. This effect, therefore, so nearly balances the other, that the general result may be thus stated. The eclipses, as described in the paper to which I have referred, are accounted for almost equally well, either by Hansen's Tables, including the equation now rejected, and including also Hansen's secular acceleration; or by Hansen's Tables, deprived of that equation, and combined with a secular acceleration diminished to the extent which I have indicated.

My confidence, however, in the certainty of chronological results, derived from lunar calculations, is in some measure shaken by the character of the unexplained inequality in the epochs of Moon's mean longitude, to which I have called attention.

Royal Observatory, Greenwich,
1873, *October* 8.

On the Correction to Hansen's Semi-Diameter of the Moon from Occultations of Stars. By E. Neison, Esq.

In the Appendix to the volume of *Greenwich Observations* for 1864, Sir George Airy has shown, from the computations of Mr. Breen, that the value for the Moon's semi-diameter given by occultations of stars by the Moon, is less than the apparent telescopic semi-diameter by more than two seconds of arc. As the value for the mean telescopic semi-diameter was derived from the Greenwich observations, and was regarded as an accurate determination of the Moon's semi-diameter as viewed with the Greenwich instruments, this difference was looked upon with considerable interest.

Hansen's value for the semi-diameter of the Moon being regarded as expressing very accurately the true semi-diameter of the Moon, it became a matter of interest to determine whether any similar difference occurred with regard to this value and that given by occultations of stars by the Moon. It was, moreover, desirable to extend the period embraced by Mr.

Breen's computation from the end of 1860 up to 1870, the latest period available; thus increasing the number of phenomena in each class, which was rather small for the purpose of an investigation of this kind.

The method adopted was the same as that described by the Astronomer Royal in the Introduction to Mr. Breen's Results (*Greenwich Observations*, 1864, Appendix 1), and therefore does not require to be detailed here.

The corrections to the tabular R.A. and N.P.D. of the stars were obtained from the following sources. For the years 1861 to 1868, from the Annual Catalogue in the *Greenwich Observations* for each year, and from the Greenwich Seven Year Catalogues for the epochs of 1860 and 1864; the mean being taken by giving a proportional weight in each case according to the number of observations. For the years 1868 to 1870 the places were taken from the *Greenwich Observations* for those years, and from the New Seven Year Catalogue, the mean value likewise being taken, with due allowance for the number of observations. The places of two stars had to be derived solely from the Greenwich Twelve Year Catalogue, and those of two others in the first division depend on observations made during 1868 and 1869, contained in the *Greenwich Observations* for those years.

The corrections to the tabular places of the Moon are derived in every case from the observations made during the same day at Greenwich, and are obtained by changing the sign of the tabular errors of the Moon's place given in the *Greenwich Observations*. With ten exceptions the mean of the errors in the Moon's place, according to the transit-circle and altazimuth was taken; and in these exceptional cases the value given by the altazimuth has been adopted, as no observations with the transit-circle had been made.

On substituting these values of e, f, x, and y in the well-known final equation given in the *Greenwich Observations*:—

$$\text{Tabular semi-diameter} - \text{computed semi-diameter} = \mathrm{A} \times e + \mathrm{B} \times f + \mathrm{C} \times x + \mathrm{D} \times y;$$

A B C D being the coefficients multiplying the corrections to the places of the Moon and star, and adding the sum of the products to the quantity on the left side of the equation with its sign changed, we have at once the correction to the tabular semi-diameter.

As during the years 1862 to 1870 the values used in the Greenwich reductions of occultations are Hansen's, this gives, without anything further, the correction to Hansen's semi-diameter. For the year 1861, however, the values for the Moon's semi-diameter and parallax differ from Hansen's, and it becomes necessary to apply a further correction. The Greenwich semi-diameter used exceeds Hansen's by $1''\cdot21$, which must therefore be subtracted from the correction given by the above equation.

Hansen's value for the parallax is less than the value employed

for the tabular places of the Moon in the Greenwich reductions of occultations for 1861, by $2''\cdot 7$: giving a value to the function denoted *m* in the Greenwich notation of $-0\cdot 7894$; which, multiplied by its proper coefficient, must be applied to the correction obtained as described above.

The resulting corrections to Hansen's semi-diameter were finally classified under four heads; but it was found necessary to omit three or four in which the correction was greater than eight seconds, the reason for which, with one exception, being found in the remarks attached to the observations.

The mean of the results is as follows:—

I. Disappearance of stars at the Dark Limb.

Mean correction to Hansen's semi-diameter from 35 observations $-1''\cdot 70$.

II. Disappearance of stars at the Bright Limb.

Mean correction to Hansen's semi-diameter from 11 observations $+1''\cdot 81$.

III. Reappearance of stars at the Dark Limb.

Mean correction to Hansen's semi-diameter from 20 observations $-0''\cdot 36$.

IV. Reappearance of stars at the Bright Limb.

Mean correction to Hansen's semi-diameter from 10 observations $+1''\cdot 31$.

The observations of the phenomena in Classes II. and IV. are of so very delicate a nature, and are comparatively few in number, that less reliance can be placed on them than on the other two: or to use the words of Sir George Airy in his Introduction to the Results of Mr. Hugh Breen's Computations:—"Of these means, the first and third are (from the nature of the observation) very far superior to the second and fourth, and the first is greatly preferable to the third. The second and fourth classes, as might be expected, give an occultation semi-diameter larger than the first and third class."

As a result, we have a correction to the value of Hansen's for the Moon's semi-diameter exceeding one-and-a-half seconds of arc, derived from the most reliable class of occultations, namely, disappearances at the Dark Limb; and there would appear difficulties in explaining this by the effects of irradiation. For in the discussion of the eclipse of the Sun of December 21, 1870, the observations with the great equatorial indicate that the effect of irradiation, when exerting its maximum effect in reducing the semi-diameter, only diminishes Hansen's value for the semi-diameter by half a second of arc. The true value for the Moon's semi-diameter it would appear, therefore, cannot be less than this would indicate, while the occultation semi-diameter is considerably less.

The question as to the probable cause of this difference is under investigation; and for the purpose of better treatment, a number of occultations of stars, observed at various observatories, are undergoing reduction, so as to obtain a greater number of phenomena to elucidate the question.

On the Rejection of Discordant Observations. By E. J. Stone, Esq., Her Majesty's Astronomer at the Cape of Good Hope.

In the *Monthly Notices* for April 1873, there appears a paper upon this subject by Mr. J. W. L. Glaisher. Mr. Glaisher states that the original object of his paper was to examine a criterion for the rejection of discordant observations, which I had published in the *Monthly Notices* for April 1868. I have read Mr. Glaisher's paper with care, and venture to offer the following remarks on the subject.

The objections to my criterion appear chiefly to resolve themselves into a preference for a general method of treating observations, which Mr. Glaisher has developed at considerable length. This method is regarded as a complete method of treatment of observations by the theory of errors, and is considered to render the rejection of anomalous results unnecessary. But as Mr. Glaisher supposes that before his treatment is applied 'obvious mistakes' are rejected, and my criterion refers only to the rejection of mistakes, I can hardly understand in what manner the proposed treatment is to render unnecessary my, or some other, criterion for the rejection of mistakes. There are no distinct marks upon the mistakes which appear amongst the records of observations, by which they can be infallibly recognized and rejected.

With respect to the statement made by Mr. Glaisher 'that the disqualification of an observation must depend solely upon the magnitude of its supposed deviation from the truth,' I must confess that this statement appears to me obviously incorrect. The deviations from the truth which may be found amongst observations will depend upon the accuracy of the observations. Deviations will frequently appear in one class of observations made with one class of instruments, which would in the same class of observations made with superior instruments be, I presume, amongst those 'obvious mistakes' which Mr. Glaisher would at once reject. When mistakes appear amongst the records of observations, they can only, I believe, be recognized by a consideration of their deviation from the supposed truth *in comparison with the average discordances of the class of observations under consideration*, and then they can only be recognized with more or less probability and not with absolute certainty.

I believe that most observers and experimentalists will admit

that the number of mistakes which will be found amongst the records of observations will depend upon the carelessness of the observer, the complexity of the observations, the clearness with which the scales, or micrometer-heads, to be read are divided, and the circumstances of illumination of these scales under which the observations have to be made. I believe also that it will be admitted by such persons, that a discordant result, which appeared amongst the records of observations, was more likely to have arisen from a mistake, if the observations were made under unfavourable circumstances, than if made under more favourable circumstances. The only assumptions upon which my criterion for the rejection of anomalous results is based are contained in the above statements.

I assume that a particular person, with definite instrumental means and under given circumstances, is likely to make, on an average, one mistake in the making and registering n observations of a given class. The probability, therefore, that any record of his of this class of observations as a mistake is $\frac{1}{n}$. From the average discordances amongst the registered observations of this class we can find the probable error of an observation in the usual way, and also the probability of an error greater than a given quantity, as C. Then my principle simply amounts to this,—that if the probability in favour of a discordance as large as C is less than that of a mistake, or $\frac{1}{n}$, I am not only justified in rejecting, but, as a reasoning animal, required to reject the discordant observation. I do not pretend that this method affords a principle by which mistakes can be recognized with absolute certainty. It is quite possible, in accordance with my views, that mistakes should occur, too small to be rejected by my criterion. It is quite possible that the result rejected as a mistake may arise from an unusual aggregation of the results of several independent sources of error, each of which has acted upon the other observations. But what I do maintain is (1) that the rejection of a mistake is of far more importance than the omission of an observation from a group. One mistake may destroy the evidence afforded by a considerable group of observations. (2) That the rejection of results which appear amongst the records of observations can only be justified by a consideration of opposite probabilities. (3) That when the evidence in favour of a result arising from the action of the ordinary sources of error is less than that, it arises from sources of error different from those in constant action, and with those results, if I could separate them, I have no concern; then I am not only justified but required to reject such discordant result.

I have never seen any clear objections raised to the principle upon which my criterion is based. I can, however, quite understand that anyone may have serious doubts, before trial, of its practical value. It is no doubt true that the value of n for a

given observer and class of observations can rarely, if ever, be determined with perfect accuracy. If, therefore, an accurate value of n be required in practice, the method would, as a practical guide, fail. But such is not the case. I consider that one of the most important results of my paper was to show that no assumptions could be made respecting the number of observations which, on the average, were made with one mistake, without leading to a limit beyond which there was a greater probability of an error arising from a mistake than from the average run of errors, and that with any suppositions that could be admitted in practice, the limits of rejection differed but slightly. If the probable error of a result is equal to $0''\cdot477$, and we assume an observer to commit one mistake in the making and recording 100 observations, I have shown that the limit of rejection would be $1''\cdot9$; but if we assume that the observer only commits one mistake in the making and recording of 1,000 observations, the limit of rejection would only be shifted to $2''\cdot3$. Nor is this all. If my criterion be accepted, it follows that for a class of observations in which the liability to mistakes remains sensibly constant, whilst the probable errors of the results vary; as for instance, Polar Distances at different Zenith Distances, then the limit of rejection varies directly as the probable error. Thus assuming one mistake in the making and recording of 500 observations, we should have no more grounds for the rejection of a discordance of $17''\cdot6$ for a Zenith Distance a little greater than 87° than for the rejection of a discordance of $2''\cdot2$ at a Zenith Distance about 26°.

As Mr. Glaisher thinks that "It is pretty clear that it would be a good deal easier to choose a limit of retention at once than to guess a value of n and deduce one," I would ask him to consider the above case and the following. Is it easier to decide for Zenith Distances about 87° with probable eror $3''\cdot82$, whether the limit of rejection should be $7''\cdot6$ or $17''\cdot6$, or to decide whether the observer is likely to be so grossly careless as to make one mistake in every five observations, or so careful as to only make one in five hundred? I have very little doubt in which way this question would be decided by most minds. My criterion, if its principle be accepted, shifts the decision of the first question upon the second. Our mathematics, after all, can do no more for us than to bring the decision of complex questions to more simple forms. This, I believe, my criterion effects to a very considerable extent.

There is one point connected with the applications of this criterion to which I should wish to call attention. It is quite conceivable that in certain cases the number of rejections by an application of the criterion would be greater than could be admitted on the supposition of the discordances arising from mere mistakes. Such a case would have arisen in the discussion of Star places before the discovery of aberration, and in all similar cases. Here, if the number of rejections pointed out by

the criterion is greater than can be admitted on the supposition of mere mistakes, we are put, with greater or less certainty, according to the number of cases of rejection pointed out above the number of admissible mistakes, upon the trace of sources of error which act with variable intensity upon the different results or upon the discovery of new laws of nature.

Before offering any remarks upon the method of treatment of observations which Mr. Glaisher advocates, I must call attention to some miscellaneous remarks scattered over his paper, which appear to me to indicate, that, although Mr. Glaisher accepts the results of the ordinary theory, he does not accept the principles upon which alone, so far as I am aware, these results can be proved. For instance, I entirely demur to the principle of Mr. Glaisher's remark—"Now, errors may arise in a perfectly legitimate way (that is to say, by the accumulation of smaller errors in the way contemplated by the theory and independently of the observer's care), and yet their retention may be quite as disastrous as if they had been mistakes made by the observer." The principle of this statement is, in my opinion, entirely opposed to the first principles upon which the law of facility generally adopted is based. I presume, however, that this statement must be connected with another—"The continuous errors are bound to look after themselves if the number of observations is infinite, but not otherwise, and in that case there is no distinction between continuous and intermittent errors at all."

This is the mere language of the schools, to which an answer, equally satisfactory, might be given in the same language. Although it is true that the continuous errors will not look after themselves until the number of observations is infinite, yet an infinity of a different order is required before the intermittent errors will be found to do so. Practically, however, we have nothing to do with infinities of observations.

It is only so far as we can legitimately assume that the mutual destruction of errors which would take place in an infinity of observations will take place sensibly in the number of observations with which we have to deal in practice, that we can transfer the theorems of the calculus of probabilities to the discussion of the results of observation.

In such cases it is most certainly not true, because we may assume as a close approximation to truth that this mutual destruction of errors takes place for the continuous errors, we may with equal truth assume that it takes place for intermittent errors arising from causes which will act only once in two or three hundred observations, and which, when they do act, may produce errors enormously larger than those arising from the causes in continuous action. These remarks meet, I believe, all Mr. Glaisher's statements respecting my criterion. (*Monthly Notices*, vol. xxxiii. pp. 398 to 400.) We have not, in practice, to deal with such numbers of observations that the average

number of observations with one mistake is a very small fraction compared with the number of observations discussed.

Unless this is the case, Mr. Glaisher's remarks on page 400 have not, in my opinion, the slightest bearing upon the practical aspects of the question. In my paper of 1868 I pointed out what I believe is a valid objection to a criterion proposed by Professor Chauvenet, viz.:—that whenever the probability of an error in n observations was less than $\frac{1}{2}$, the corresponding observation should be rejected.

My objection was thus stated:—"In $2n$ such observations we ought reasonably to expect an error greater than or equal to x. In n observations, therefore, we ought not to be surprised at the appearance of such an error, and certainly its appearance would be no ground for the assumption of some disturbing cause of error." To this Mr. Glaisher appends the following remark:—"Taking the words literally, it is enough to reply that the question is not how far we are surprised at such an error having been made, but whether it should be retained or not." I do not know whether Mr. Glaisher wishes it to be understood that he thinks the objection I have made unsound; but I presume he would hardly have extracted my remarks upon this point and appended his own unless he did think so. If such be the case, I fear that nothing I can say will make the point clearer to him. My objection simply amounts to this—that if the odds are fairly two to one against an event coming off, and yet the event does come off, we have no right to assume from this alone that unfair practices have been resorted to. There is a statement made by Mr. Glaisher (*Monthly Notices*, vol. xxxiii. p. 402), to which I will now call attention:—"Another point deserving notice is the statement, usually received as self-evident, that positive and negative errors are equally probable. In certain conceivable cases this seems, at first sight, not to be the case; for, suppose one were to estimate the area of a field containing, say, two acres, then it would be quite possible to say four acres, or even six, but absurd to say zero or minus two (a case of this kind was pointed out to me by a pupil in my lectures this term, as an exception to the rule). It is a sufficient justification of such an apparent exception to observe that an estimation (or guess) is not of the nature of an observation, as contemplated in the theory of errors; but still the analogy is close enough to make the discrepancy worth remark." Now, I must confess that I have never heard any one before maintain that positive and negative errors are always equally probable. There are such things as systematic errors recognized in practice, *i.e.* errors which, for an observation made with a particular instrument in a particular way, will always enter into the result with the same sign. I believe that it is usually assumed that it is only for those classes of errors for which positive and negative results are equally probable that theorems which depend upon the ordinary law of facility of errors can be appealed to as means of the elimination of error in the result.

But this can hardly be Mr. Glaisher's meaning, for such a view would have prevented the difficulty mentioned from being raised.

Assuming the law of facility

$$\frac{h}{\sqrt{\pi}} \cdot \varepsilon^{-h^2 x^2} dx$$

for an error between x and $x + d\,x$

and denoting the n independent results by a_1, $a_2 \ldots a_n$; $a_1 - x$, $a_2 - x$, &c., by x_1, $x_2 \ldots x_n$, and the corresponding measures of precision by h_1, $h_2 \ldots h_n$, we have for the probability u of the coexistence of the errors x_1, $x_2 \ldots x_n$

$$u = \pi^{-\frac{n}{2}} \varepsilon^{-(h_1^2 x_1^2 + h_2^2 x_2^2 + \ldots)} h_1 . h_2 \ldots h_n (dx)^n$$

Then assuming $h_1 = h_2 \ldots = h_n = h$ a constant, Mr. Glaisher deduces, in accordance with the ordinary theory,

$$x = \frac{a_1 + a_2 \ldots + a_n}{n}$$

With this result, the probabilities in favour of x_1, $x_2 \ldots x_n$ are computed.

Mr. Glaisher then proceeds to obtain a result which he considers more probable than the arithmetical mean, as follows. Having found the probabilities (on the assumption made, viz.:— that $h_1 = h_2 \ldots = h_n$ in favour of errors x_1, $x_2 \ldots x_n$ to be p_1, $p_2 \ldots p_n$ he weights the results in accordance with the probabilities, and determines x from the condition that it shall make a maximum of

$$\varepsilon^{-[p_1^2 (a_1 - x)^2 + p_2^2 (a_2 - x)^2 + \ldots]}$$

This value of x is considered as more probable than the arithmetical mean, and Mr. Glaisher recommends that the process should be continued until the consecutive values of x come out sensibly the same.

I admit that whenever a large discordance from the arithmetical mean is an indication of the action of sources of error of an abnormal character, less weight should be given to the corresponding observation; but instead of regarding Mr. Glaisher's method as mathematically complete, I regard it as mathematically unsound, even upon the assumptions made.

If we are to ignore all *a priori* considerations, and to adopt as the most probable result the value of x which makes

$$u = \pi^{-\frac{n}{2}} \varepsilon^{-(h_1^2 x_1^2 + h_2^2 x_2^2 + \ldots)} h_1 . h_2 \ldots h_n (dx)^n$$

a maximum, where h_1, $h_2 \ldots h_n$ are to be considered, as Mr. Glaisher supposes, unknown quantities whose values are to be assigned from a consideration of the mere run of the numbers in the question before us. Then clearly h_1, $h_2 \ldots h_n$ are

variables, and must be treated as variables throughout the mathematical process for finding the maximum value of u.

Differentiating u and equating the differential to zero, we have

$$\frac{Du}{u} = 0 = 2\,(h_1{}^2x_1 + h_2{}^2x_2 + h_3{}^2x_3 + \ldots)\,dx$$

$$+\,dh_1\left(\frac{1}{h_1} - 2\,h_1x^2{}_1\right) + dh_2\left(\frac{1}{h_2} - 2\,h_2x_2{}^2\right) + \text{\&c.}$$

$$\therefore\ h_1{}^2x_1{}^2 = h_2{}^2x_2{}^2 = h_3{}^2x_3{}^2, = \text{\&c.} = \tfrac{1}{2}$$

and

$$h_1{}^2x_1 + h_2{}^2x_2 \ldots + h_n{}^2x_n = 0$$

or

$$x = \frac{h_1{}^2a_1 + h_2{}^2a_2 + \ldots}{h_1{}^2 + h_2{}^2 + h_3{}^2 + \ldots}$$

From which we deduce for the determination of x the equation

$$\frac{1}{x_1} + \frac{1}{x_2} + \frac{1}{x_3} + \ldots + \frac{1}{x_n} = 0 \quad (1)$$

The value of x which makes u an absolute maximum must be contained amongst the roots of the equation (1), and this equation will always have one real root which will make u a maximum.

Such is, I believe, the correct solution of the question on the assumptions made, that the weights of each observation are to be deduced entirely from the run of the observations under consideration without any regard to *a priori* probability.

On the Limit of a Possible Lunar Atmosphere.
By E. Neison, Esq.

In a preliminary note in the *Monthly Notices* of the Royal Astronomical Society (Vol. xxxiii. p. 464), attention was drawn to the circumstance that no proof of the absence of a sensible lunar atmosphere had as yet been offered, but only that it did not exceed a certain undecided limit. The question possessing no little interest, and being of importance with reference to the present constitution of the Moon's surface, it appeared desirable to fix the limit as satisfactorily as possible within which a lunar atmosphere was possible. For there can be no doubt but that any atmospheric envelope to the Moon, however small in density, must exert a considerable influence on the surface, and enable actions to take place otherwise impossible.

There can be no question but that the main circumstance limiting the density of any possible atmosphere is the refraction of the rays of light it would cause. From what we know on the subject, as there can be no doubt but that it must be of comparatively small density, it is evident we have only to deal with the hori-

zontal refraction, as the refraction must practically vanish for any beyond very low altitudes.

For the purpose then of ascertaining the limiting value for any possible lunar atmosphere, it will be requisite to determine within what limits there may be a horizontal refraction. Taking everything into consideration, the process best adapted to detect this horizontal refraction would be the different values given by a telescopic and occultation determination of the semi-diameter of the Moon. After having determined to within what value the horizontal refraction is limited by the known values of the lunar semi-diameters determined by these means, it would be possible to ascertain if any other phenomena still further limits it.

The Astronomer Royal has shown (*M. N.* xxv. p. 261, *Greenwich Observations*, 1864, Appendix I.,) as the result of the computations by Mr. Breen of the Moon's occultation semi-diameter, from observations of 295 occultations, that the following corrections are given by it to the Greenwich telescopic semi-diameter:—

By disappearance of stars at the dark limb	=	$-2''\cdot00$
By reappearance of stars at the dark limb	=	$-2\ \cdot40$

The similar observations at the bright limb give greater values for the semi-diameter, as might *a priori* be expected, both from the extreme delicacy and difficulty of the observations, and from the irradiation of the limb being apt to extinguish the light of the star before it reaches the actual limb. As Sir George Airy remarks: "We cannot be sensibly in error in saying that the Moon's occultation semi-diameter is less than the Moon's telescopic semi-diameter by $2''\cdot0$."

This excess is generally attributed to the results of irradiation at the bright limb increasing the telescopic semi-diameter, as the Greenwich value for this, referred to above, is considered a very exact determination; and although this may be the correct explanation, there can likewise be no doubt but that a portion, if not the whole, may possibly arise from a lunar atmosphere. Hansen's value for the semi-diameter of the Moon is regarded as being an exact determination of the true diameter, and is considerably less than the Greenwich telescopic semi-diameter; a reduction, however, of the occultation of stars for the years 1861 to 1870, gives a correction for the most favourable and accurate class of phenomena of $-1''\cdot70$ to this value, and the greater portion of this correction it appears cannot arise from the effect of irradiation. As a certain amount of irradiation must occur, and can but affect the telescopic semi-diameter, we cannot assume the whole correction to the telescopic semi-diameter as being due to the possible existence of a lunar atmosphere; and it would appear more satisfactory to allow for this, and say that these results show that an atmosphere may exist on the Moon capable of exerting a horizontal refraction of about one second of arc. Apart from other evidence, the observa-

tions of solar eclipses; especially those of 1860 and 1870 at Greenwich, where we have the effect of irradiation at its maximum, and exerted in diminishing the apparent lunar diameter; show that it is impracticable to ascribe to irradiation the excess of observed and computed semi-diameters over the occultation semi-diameter, as they unite in giving a maximum correction to Hansen's semi-diameter from the effects of irradiation of $-0''\cdot5$.

It remains now to determine on the basis of a possible horizontal refraction of one second of arc, the probable condition of the supposed lunar atmosphere. For the purpose of computing the horizontal refraction it will only be necessary to ascertain a probable law of decrease of density, as it has been shown very different rates of decrease give sensibly the same horizontal refraction, and considering the smallness of the horizontal refraction to be determined, it will be evident that considerable errors on the Earth would be insensible upon the Moon. As the rate of decrease of density depends upon the rate of decrease of temperature, and as there is no possibility of determining this, it will be necessary to assume some hypothesis. Reasoning from analogy, and taking into consideration the much slower decrease in density from the action of gravity, and the greater mobility from the decrease in density, it would appear we must have upon the Moon, not only a more equable decrease in density, but a greater uniformity in temperature, and consequently a slower decrease. It will be assumed that near the surface the decrease of temperature for increasing heights will be nearly equable, and slowly decreasing to a definite temperature at the upper limit of the atmosphere, an hypothesis similar to that of Laplace for the Earth. (*Mécanique Céleste*, tome x. ch. 1, § 7.) It may here be remarked that the more rapid the decrease of temperature the less the horizontal refraction becomes.

For the purpose of determining the refraction, it will be assumed that the atmosphere is unlimited in height, and that as before stated the temperature decreases slowly to a fixed minimum as the altitude increases. Unlike the Earth, the Moon suffers great variations in the temperature of its surface. Lord Rosse's researches show the temperature of the surface of the Moon to be very variable, reaching probably a maximum temperature of nearly two hundred degrees centigrade, and falling probably considerably below zero. This last is readily seen, considering the long lunar night, and the slight retarding influence exerted by a rare atmosphere free from moisture. As the temperature of space must be constant, it is evident this difference in the surface temperature will materially alter the constitution of the atmosphere and the amount of the refraction. We cannot from Lord Rosse's figures deduce any law for determining the variation of temperature, but it may approximately be taken as varying as the sine of the solar altitude. Consequently the maximum surface temperature of the following limb, would be at about the eighth day of the Moon's age, and the minimum about two days before full, while for the

other limb the similar periods would be the day after the third quarter and two days before full.

For the temperature of space, and therefore of the upper portion of the atmosphere, we have according to Fourier and Hopkins, about $-50°$ centigrade, which value is adopted; but it is evident that by lowering this value we have not only a less horizontal refraction, but a greater mass of atmosphere, and therefore even more favourable conditions than those assumed. It is true by taking the temperature near $-266°$ centigrade it renders the determination of the height of the atmosphere easier; but we have no reasons for assuming this to be in any sense probable; and Ivory has shown the height of the atmosphere does not sensibly alter the refraction. Upon, then, this value for the temperature of space we have for the minimum surface temperature of the Moon $-30°$ cent. We must refer to one or two points before proceeding further. In considering the conditions of the atmosphere, the first mile from the surface is excluded, as liable to great local variations, and as not taking part in the refraction. This last is apparent, as for one hundred miles in width at the limb, the numerous mountains undergo no sensible diminution in height, and so must form a projecting ridge of quite this height. To determine the height of the atmosphere, as Laplace has shown that the height is of necessity limited, we must take the following consideration; for although to determine the refraction we may take the heights as unlimited, for other purposes we must determine its probable limit. We have physical reasons for believing that after the decrease of density has reached a certain amount, the elasticity of the atmosphere is destroyed at a medium low temperature, and this we must suppose reached at the limit of our own atmosphere. By taking then the density here as the minimum density, we can fix approximately the height of the lunar atmosphere as the point where this density is reached. Various heights for our atmosphere have been fixed, at from twenty-five to eighty miles, and by taking one hundred miles, it may be regarded as the extreme. Calculating upon Laplace's theory the density of the atmosphere at this height, we have, taking the surface density as unity, a number whose common logarithm is $\overline{12}\cdot3$. As the Moon's atmosphere will be about one five-hundredths of the density of that of the Earth at the surface; we may take the Moon's atmosphere as ceasing when its density is but the number whose logarithm is $\overline{9}\cdot0$ of the surface density. It is evident, however, that variation in this will exert very slight effect upon the density at the surface.

The physical conditions of an atmosphere upon the hypotheses adopted have been investigated by Laplace, Ivory, and Plana; but that of Ivory has been selected as not only generally preferred, but as from its simplicity admitting of being readily modified so as to suit conditions found on the Moon. Ivory's Memoirs are contained in the *Philosophical Transactions* for 1823 and 1838, and simple integrations of his equations are given by

Brünnow (*Spherical Astronomy*), and Bruhns (*Astronomische Strahlenbrechung*, pp. 148–159); they can also be readily deduced from Laplace's equations (*Mécanique Céleste*, tome x. ch. 1, § 7), and to one of these reference must be made for the demonstration of the equations used.

The notation used is as follows:—

a = radius of Moon.

x = altitude above surface of any point P.

δ, p, t = density, pressure, and temperature at P, and δ_0, p_0, t_0 ditto at surface.

l_0 = height of column of air of density δ_0, that at temperature t_0 would exert under the action of the Moon's gravity, a pressure equal to p_0.

$\alpha = 0{\cdot}000294\delta_0'$, putting this last in terms of the density of air at the surface of the Earth, and at 0° cent. and 760 millimetres pressure.

$\epsilon = 0{\cdot}003665$, its usual value as determined by Regnault.

Remembering that the height is to be considered unlimited, and as upon the hypotheses taken where m varies as the height of the atmosphere,

$$\frac{\delta}{\delta_0} = \left(1 - \frac{u}{m+1}\right)^m$$

we have for m infinite.

$$\frac{\delta}{\delta_0} = e^{-u},$$

and

$$\frac{p}{p_0} = e^{-u}\left(1 - f + fe^{-u}\right), \text{ and } \frac{p\,\delta_0}{p_0\,\delta} = 1 - f\left(1 - e^{-u}\right) = \frac{1 + \epsilon t}{1 + \epsilon t_0}$$

where

$$x = l_0\left\{(1-f)\,u + 2f\left(1 - e^{-u}\right)\right\}$$

where f is the function on the value given to which depends the rate at which the temperature decreases. It is evident from the third of these that as f increases the rate of decrease of temperature decreases, and that for $f = 0$ we have a uniform temperature, and that it cannot exceed unity.

Finally, for the horizontal refraction we have, putting r for the refraction and θ for zenith distance,

$$dr = \alpha(1+\alpha)\sin\theta\left\{\frac{d\left(1 - \frac{\delta}{\delta_0}\right)}{\sqrt{\left\{\cos^2\theta + 2\frac{x}{a} - 2\alpha + 2\alpha\frac{\delta}{\delta_0}\right\}}}\right\}$$

which equation can be expanded into terms of the well-known

form $e^{T^2} \int_T^{\infty} e^{-tt} dt$, which can be readily integrated and reduced

to the approximate form as under, with only insensible errors.

$$r = a(1+a) \frac{\sqrt{\pi \frac{a}{l_0}}}{\sqrt{2}} \left\{ 1 - \left(f - a\frac{a}{l_0}\right)\left(\sqrt{2} \cdot 1\right) + f\left(\frac{3}{2} - \sqrt{2}\right) \right\}$$

The term f can readily be deduced from the fact that it must be such as to make the temperature equal to that of space at the upper limit of the atmosphere, and not affect the surface temperature; and as from one of the equations above, we have $f = 1 - \frac{1 + \varepsilon t}{1 + \varepsilon t_0}$ approximately, by putting for t and t_0, the temperatures of the upper and lower limit of the atmosphere respectively, f is at once determined. It is also apparent that for the upper limit of the atmosphere, taking it as fixed by the value for the density before fixed, u will be approximately twenty, or the height of the atmosphere, for all surface temperatures remain sensibly constant, at a height slightly above half the Moon's radius.

Now, determining the surface density, so that the horizontal refraction at 25° centigrade will not exceed one second of arc, and be a convenient number for calculation: this will be found best represented by 0·0025 of the Earth's surface density at its standard pressure and temperature, or 760 millimetres and 0° centigrade. Putting then these values in the formulæ for the horizontal refraction, we have it amount to the following for the surface temperature stated:—

Surface temperature.		Horizontal refraction.	
− 30° centigrade	...	1″·27	Mean temperature of Dark Limb.
0° „	...	1·03	
25° „	...	0·88	
100° „	...	0·59	Mean temperature of Bright Limb.
200° „	...	0·39	

It will be seen that these results are confirmatory of the cause of the difference between the occultation and telescopic semi-diameters, having their origin in the presence of a lunar atmosphere.

We have taken with Sir John Herschel the actual retardation at the limb, at its generally received value, namely, the horizontal refraction; and not twice this, as has been done. It can readily be shown that the retardation of an occultation is less than twice the horizontal refraction, as follows:—Although the angle between

the paths of a ray before and after refraction is evidently twice the horizontal refraction, yet it is apparent that the ray that would reach the observer is not that which would reach him in the absence of an atmosphere, but one lying much further from the limb; while that which would otherwise reach the observer after grazing the limb in the absence of a lunar atmosphere, would, if it were present, strike the surface of the Moon, and not reach the observer.

It remains now to notice the objections that have been raised to the existence of a lunar atmosphere, and it is evident that with one or two exceptions, as they were all directed against an atmosphere usually as dense, or even denser than our own, they are valueless as directed against one only one four-hundredths of this density. The phenomenon referred to by Mr. Proctor, in his Work on the Moon, as preventing the occultation of a star, could only arise from a lunar atmosphere much greater than our own, even were it not prevented from the rays from the Moon after refraction being divergent and not convergent, as he assumes in his illustration. It will also be apparent that for the density of the supposed atmosphere, no distortion of a star before occultation could possibly occur, and the same applies to the occultation of a planet such as *Jupiter* or *Saturn*, the maximum effect would be to increase the size of the planet by about one-thousandth; but in no case distort it. Dr. Huggins' observation (*M. N.* vol. xxv. p. 60) is evidently by no means delicate enough to detect the very slight effect capable of being exerted by an atmosphere of the density supposed. The effect of a lunar atmosphere upon an eclipse of the Sun, would, if of the density assumed, be sensibly the same as a diminution of the semi-diameter by about one second, or would be lost in the effects of irradiation. Finally, it can hardly be seriously urged that it could materially interfere with the observation of the reversal of the dark lines in the solar spectrum, considering the smallness of the horizontal refraction, and the extremely minute amount of scattering of the solar rays the supposed atmosphere could effect. No known objection yet raised appears to limit a possible lunar atmosphere more than the difference between the occultation and telescopic semi-diameter.

The real dimensions of the atmosphere shown to be possible upon the Moon's surface, can be best shown by the fact that its total weight above one square mile is about four hundred thousand tons; and that it bears nearly one eighth of the proportion of the Moon's mass, as the Earth's atmosphere does to the Earth's mass. The consideration of these features, however, had better be deferred until the fact of the presence of an atmosphere has been demonstrated.

In conclusion it is to be observed that the purport of the present paper has been to show that it is *possible* that a lunar atmosphere *may* exist, and to define its probable condition; the task of showing that a lunar atmosphere *does* exist requires different and more certain evidence.

On an alleged Variability of the Sun's Diameter. By Dr. Auwers.*

(*Abstract by W. T. Lynn, B.A.*)

Lindenau found in reducing the observations of the Sun with the transit instrument at Seeberg, in the years 1808 and 1809, differences in the observed diameters which he thought could not be explained by errors of observation, especially as they seemed to be of a periodic nature. By a discussion of the meridional observations at Greenwich from 1750 to 1755, and from 1765 to 1786, he obtained an apparently complete confirmation of the reality of these variations and their periodical charactér, and was able satisfactorily to represent the observations by the hypothesis that the Sun was an ellipsoid rotating above its major axis; the compression, according to his calculation, amounting to something between $\frac{1}{279}$ and $\frac{1}{140}$.

This was communicated to the number of Zach's *Monatliche Correspondenz* for June 1809. In the following number of that periodical, Bessel remarked that the variations noticed in the Sun's apparent diameter at Greenwich could be explained by a periodical shifting of the wire-frame of the instrument in reference to the plane of the focus. Lindenau's investigation has not led to any further criticism; more accurate observations showing no deviation in the Sun's apparent disc from that of a circle, and his theory of a compression of the Sun's body was set aside as resulting from errors in the early observations.

Recently, however, Father Secchi, apparently unaware of this investigation of Lindenau, has conjectured that the effect of the active forces in the Sun, which are made known to us by the variable formations on its surface, may produce changes of volume in the masses of luminous gas, perhaps perceptible in accurate observations of the Sun's diameter. He, in consequence, caused his assistant at the Observatory of the Collegio Romano, P. Rosa, to make regular observations from July 1871, of the duration of the Sun's transit with the meridian-circle; the result of which appeared to confirm his suspicion of considerable variations in the Sun's diameter, and a close connection of this with the indications of the force forming the spots and protuberances. The variations in the apparent diameter were greatest when the activity of the forces was greatest; and (as Secchi asserted) these variations often exceeding 3″ in amount were greater than, and could not be explained by, mere errors of observation. Their sometimes regular change also seemed to negative the latter idea; and at Secchi's instance, observations were afterwards made at the Palermo Observatory, which appeared to confirm the result obtained at Rome. They agreed in showing that "the greater diameter was observed at those times at which the number of the spots and protuberances was less."

* From the *Monatsbericht* of the Royal Academy of Sciences at Berlin for May 1873.

Secchi proceeded to compare the Roman observations (187 in number) with the heliographical latitudes of their end-points, which, during the year (1871 July to 1872 July) of observation, passed four times through the values from 0 to $\pm$ 26°. The curve formed from the observed values thus obtained appeared to him to be regular and confirmatory of each other in the four divisions they contained. He assumed therefore "a maximum of diameter between the equator and $\pm$ 6° of heliographical latitude, its amount being $32'3''\cdot74$ and a minimum between $\pm$ 21° and $\pm$ 23° of latitude, the amount being $32'2''\cdot18$; the difference of the two being $1''\cdot56$, five times as great as the probable error of a single observation, whilst the value of the maximum was established by 31, and that of the minimum by 22, observations."

While Secchi does not attempt to claim these results, founded on only one year's observations, as final, he yet maintains them to be undeniable deductions from the observations made. Dr. Auwers, however, asserts that the foundations of his theory are so unreliable that the consequences deduced from it must be, for the present at any rate, absolutely rejected. In the first place, it is in opposition to all experience in the nature of errors of observation, to make the deviations of the separate observed transits from their mean the only measure of the possible errors. That greater fluctuations show themselves in the observed diameters than would correspond to these deviations can, up to a certain limit, only be regarded as a proof that the determination of the probable error of observation is incomplete; and that this limit has very probably not actually been exceeded by the fluctuations in the Roman observations is shown by an investigation,* by Herr Wagner, of Pulkowa, of a series of observations evidently surpassing those at Rome in their internal goodness. The observation of a transit of the Sun's limbs is affected to such an extent in this instance, by an element totally left out of account by Secchi,—namely, the state of the atmosphere and the quality of the image produced by its condition—that differences occur between means from repeated observations of the Sun's diameter made with the great transit instrument at Pulkowa, in good, and others in bad, states of air, of from 3″ to 4″, so that differences of from 5″ to 6″ between separate observations cannot certainly be regarded as anything surprising. The data for estimating the actual amount of influence produced in the Roman observations by the condition of the atmosphere are not furnished by Secchi, and it cannot, therefore, be decided how far this element is capable of explaining the observed fluctuations; but this circumstance is sufficient to overthrow the proof which he has deduced from those observations.

With regard to the alleged agreement between the Rome and Palermo observations, Dr. Auwers demurs to the fact; it appears,

* Published in the 1873 January part of the *Vierteljahrsschrift der Astronomischen Gesellschaft.*

indeed, that although both series assign a principal minimum on April 1872, yet a detailed examination shows that the general agreement of the deviations is scarcely more marked than the contrary. And as to the third proof of the reality of the variations observed at Rome, viz. their alleged connection with the visible fluctuations of intensity in the activity of the forces on the Sun, Dr. Auwers states that he can find no satisfactory data in the published observations for the somewhat arbitrary tabulation of Secchi, or the conclusions deduced from it. He proceeds to subject these conclusions generally to the test of a comparison with more comprehensive materials. This enquiry, he remarks, is not without an obvious importance in the question of the relative accuracy of the different methods of observation to be employed in the transit of *Venus*. For observations of which he has availed himself for this purpose, which have been made, but not yet published, at Greenwich, Neuchâtel, Oxford, Washington, Paris, Königsberg, Brussels—all taken during the same period of time as those at Rome—Dr. Auwers offers his thanks to Sir George Airy, Messrs. Becker, Main, Sands, Loewy, E. Luther, and Quetelet.

From the whole mass of observations, extending from July 1871 to July 1872, "it clearly results," says Dr. Auwers, "that Secchi's assertions concerning variations in the Sun's diameter are totally and entirely unfounded, and that the changes noticed by him are due to casual errors of observation." Of course, this conclusion is only meant to imply "that changes in the diameter, produced by such variations of activity in the superficial strata, as occur within one tenth of a solar-spot period (or generally any changes within a shorter period than this), are smaller than can be recognized within that period of time by meridional observations. The possibility remains of being hereafter able to perceive such changes, either by more delicate observations or by comparisons through longer intervals of time, such as whole sun-spot periods."

Dr. Auwers examined some heliometer observations of the Sun; but the number available is not great, and no conclusion of certainty can be derived from them. He has also put together a long series of Greenwich observations by Bradley and Maskelyne and their assistants; and some shorter ones by Bessel at Königsberg, and by W. Struve at Dorpat, with the view of ascertaining whether there is any observable connection between the Sun's apparent diameter and the state of the solar-spot period; but he does not find it possible to trace any such connection. Finally, he has compared the values of the Sun's diameter given in the last twenty years in the Greenwich Introductions (from 1851 to 1870) deduced from the Transit-Circle observations, which again show, when compared with Wolf's relative number of the solar-spot condition, "that in the fluctuations of the observed values, both for the horizontal and vertical diameters, and the difference between the two, no dependence upon the variations of the degree of activity, and, therefore, no indication whatever of the reality of these fluctuations can be perceived."

On the Variable Proper Motion of Procyon. By Dr. Auwers.

(*Abstract* by W. T. Lynn, B.A.*)

The discovery by M. Otto Struve of a small companion to *Procyon*,† which might account for the motion of that star around its common centre of gravity with some unknown body, has induced Dr. Auwers to re-determine the motion of *Procyon* by the aid of the more recent observations, with a view of ascertaining whether this were so.

In doing this he has availed himself of observations at Greenwich, Edinburgh, Cambridge, Oxford, Paris, Brussels, Washington, Williamstown, Melbourne, Santiago, Cape of Good Hope, Geneva, and Leyden. He thus arrives at a set of elements of the orbit of *Procyon* not greatly different from that which he obtained before, the period being 39·866 years; and this represents very well the whole of the observations from 1752 (Greenwich) to the present time. It confirms him in his theory of a motion of the star in a nearly circular orbit in the plane of projection in the sky, or perpendicular to the line of sight.

With regard to the object observed by M. Struve being the disturbing cause, he remarks that the angle of position of such a hypothetical companion at the time of observation (1873·24) would be 70°·8, while that which M. Struve actually observed was 87°·6. "The difference of 16°·8 (corresponding, at the place of *Procyon*, to 0″·29) is about four times its probable error, so that the assumption of the identity of this observed companion with the theoretical one is not of the most favourable kind, though the contrary is by no means proved." He goes on to say that the next spring will afford an opportunity of deciding this question, if it is possible then to observe Struve's companion-object again. For if that object is really the disturbing cause of the observed motion of *Procyon*, its position-angle at the end of March 1874 will be about 97°, whereas if it is only optically close to *Procyon*, the position-angle will be only 84°.

If the former appears to be the case, it will be necessary to attribute to *Procyon* the greatest mass hitherto known, amounting in fact to 80 times that of the Sun, whilst that of the companion would be concluded to be equal to seven masses of the Sun—the parallax being taken at 0″·24. For M. Struve's object was observed by him at a distance of 12″·4 from *Procyon*; and it follows that if it be the cause which produces the nearly circular motion of that body, it must be placed at a considerable distance from the common centre of gravity of the two.

This new and improved determination of the elements of

* From the *Monatsbericht* of the Royal Academy of Sciences at Berlin for 1873, May.

† See *Monthly Notices*, vol. xxxiii. p. 430.

the motion of *Procyon* by Dr. Auwers gives the following results:—

Epoch of Minimum R.A.	1795·629 ± 0·675 years
Annual Motion	$9^{\circ}·02993 \pm 0^{\circ}·11966$
Periodic Time	39·866 ± 0·528 years
Radius of Orbit	$0''·9805 \pm 0''·0375$

On a remarkable Nebulous Spot observed upon the Sun's Disc by Pastorff, May 26th, 1828. By A. Cowper Ranyard, Esq.

In the first volume of the MS. Observations of Geheimerath Pastorff, which were presented to the Astronomical Society by Sir John Herschel, is a drawing of a dusky spot, with a bright nucleus, seen upon the Sun's disc, on the 26th of June, 1819, and which has been generally believed to have been the comet of 1819, as seen projected upon the bright background of the photosphere. The object of the following note is to draw the attention of the Society to a very similar nebulous spot, with a small bright centre, depicted upon a drawing of the Sun's disc for the 26th of May, 1828. It is marked by Pastorff as "Hell glänzender Fleck" (bright shining spot), and its distance parallel to the horizon from the nearest point of the Sun's western limb is given as 3′; the observation was made at half-past nine in the morning, at Buchholz,* near Drossen. On the drawings of the Sun's disc for the preceding and succeeding days no such spot is registered, nor is any sun-spot or facula given as in or near its place; and, indeed, the faint nebulous haze with a minute bright centre given here, and on the drawings for the 26th of June 1819, differs totally from the type of drawings given all through the MSS. to represent sun-spots or faculæ. It would therefore be interesting to enquire whether any known comet or meteoric stream lay between us and the Sun on the 26th of May, 1828. It should be remarked that the drawings for May 1828 are on a much larger scale than those for 1819, and that the nebulous spot is both smaller and fainter than in the earlier drawing; it is therefore probable that the comet (if such it be) will turn out to be a very small one compared with that of 1819.

Remarks on Spectroscopic Observations of the Sun, made at the Temple Observatory, Rugby School, in 1871-2-3.
By J. M. Wilson, Esq., M.A., and G. M. Seabroke, Esq.

During the last two years and a quarter observations have been made at the Temple Observatory, Rugby, on the positions and forms of the solar prominences, with as much regularity as

* Lat. 52° 25′ 49″ N.; Long. 50^{m} $32^{s}·17$ E. of Paris.

weather permitted. The observations were made from August 1871, to December 1872, with the straight slit, in the usual manner, and since December 1872, with the annular slit, described by one of us, in conjunction with Mr. Lockyer, in the *Proceedings* of the Royal Society in January 1873. This method is found to be satisfactory, and to save much time.

The principal result of these observations is to show, in a very decided manner, that the solar prominences preponderate greatly in the equatorial region of the Sun; that is, they occupy the same region as the spots.

We propose therefore to offer some brief remarks on a theory of prominences and spots, intended primarily to explain their joint occurrence, but incidentally accounting for other phenomena both of spots and prominences. These phenomena are—

(1.) The travelling of spots on the Sun's disk in the direction of his rotation with a velocity increasing as the spots near the equator.

(2.) The rotation of spots.

(3.) The appearance of the penumbra as the spot nears the edge.

(4.) Spectroscopic observations on spots.

(5.) Connection of faculæ and spots.

Our reason for publishing such a speculation is that it suggests further observations, some of which require higher telescopic power, and more leisure, than we have at our command.

Let it be supposed (1) that at some depth below the Sun's surface the pressure is such that substances such as some metals are in a liquid state, though at a temperature far exceeding their boiling point under ordinary pressure. This will be the case unless the temperature of the interior of the Sun is above all their critical temperatures, in which case no pressure would liquify them; and the fact of some liquids or solids existing in the Sun seems probable from a consideration of its density.

In this case, the liquids will on the whole arrange themselves in order of density, the denser being below the lighter.

Such liquids maintain their liquid state solely in virtue of pressure, and if that is diminished they may burst into vapour. The equilibrium therefore must be unstable; a disturbance of pressure will create an outburst, originating at whatever depth liquids exist in this condition, and the outburst will travel in the direction of least resistance, i.e. towards the surface.

This result will be remarkably intensified if (2) there exist two liquids in the Sun so related, that the denser has the lower boiling point, and not capable of freely mixing with each other, like water and oil, or like chloroform and water. The effect of this combination is easily studied by taking a long glass tube, closed at the bottom, and having nearly filled it with water, pouring a few drops of chloroform into the water. If then the water is heated to near its boiling point by a flame held to the side of the tube above the chloroform, and then the chloroform

itself is heated, the water is violently ejected from the tube, the vapour of chloroform being superheated in the boiling water. Such a combination makes a kind of geyser of very sudden and violent action.

On these hypotheses, disturbances of pressure will produce vertical eruptions through the upper layers of the Sun, and throw up the chromosphere, and portions of the photosphere, into jets such as are observed. These jets will tend to be cyclonic, as we and others have observed them to be, when they cover a large area. These substances, which are carried up into the prominences, will of course be detected, as they have been, by the spectroscope. Further, if liquids or solids are carried up, there will be streaks or flashes of continuous spectrum, as seen, we believe, by Zöllner and Secchi.

Whenever there has been such an outburst, the substances must again fall to the Sun, cooled by the expansion. They will fall, and as they fall, they will produce a region of general absorption and of greater selective absorption. When any portion of the descending material is liquid—like the steam of the geyser eruption descending in water drops—the general absorption will be great; and this will be in the centre of the cooler region of descending material. Moreover, the descent of such a liquid on the heated liquids below would tend to cool those liquids, and cause less emission of light. The chloroform, to recur to the experiment above, descends in drops on the boiling water, and is again evaporated, cooling it each time it falls.

The descent from an elevated region, where the ejected materials will have received an impulse in the direction of the Sun's rotation from the atmosphere through which they will have passed, will cause the descending materials to retain some of that velocity, and thus to travel over the Sun's surface in the direction of its rotation. And in such a descending column rotation would spring up and the spot would seem to rotate.

When a spot is viewed vertically the penumbra, the region of cooler gases, will surround the umbra, the region of cooler gases and liquids; but when seen near the edge of the Sun, the umbra will be lost sight of, as lying deeper than the penumbra.

That the penumbra and umbra should have every degree of distinctness from one another, from the perfect blending, as shown in some of the Harvard sketches, to the sharp separation as usually seen with low powers, is quite compatible with this theory.

The bright bridges and the projecting tongues of light over the penumbra and umbra are portions of the photosphere drawn into the vortex of descending vapour. Or they may be sometimes secondary discharges of portions of the chromosphere thrown up in the neighbourhood of the spot. This would seem to have been the case on March 13 of this year. One of us examined the spot at 11 A.M., and reported in our notebook, 'Hydrogen seen over spot. F and C lines reversed at 11 A.M. half across

spot. The spot was then only partly divided by bridge (a) in diagram. Bridge approaching us.' The other of us happened to examine the same spot independently at 11.50, and reported, 'Hydrogen lines seen reversed across the whole spot. The dark line C was reversed, finely defined on the red side, and extending beyond the C, and indefinite on the yellow side.'

The faculæ are elevated regions, and regions in general of ascent. Prominences and faculæ have the same general cause, and will co-exist, the difference between them being simply one of intensity and of localisation to a smaller area. The spot is the region of descent corresponding to regions of ascent, and will therefore occupy the same belts of surface as prominences.

If this theory be true, the following consequences ought to follow from it, and, therefore, to be observed :—

(1.) The period of maximum sun-spots ought to be identical with that of maximum prominences. The prominences have scarcely been observed long enough to establish whether this is or is not true.

(2.) There ought to be a diminution of the height of the chromosphere in the period of maximum sun-spots. This, we have some reason to believe, is the case.

(3.) In maximum sun-spot periods, the umbra of spots should be larger relatively to the penumbra than in the periods of minimum sun-spots.

(4.) Prominences ought, on the whole, to lean in the direction opposite to that of the rotation of the Sun. From our position, relatively to the solar equator, we can only see this, if it is the case, in spring and autumn; and an examination of all our drawings establishes that this is nearly always the case. The exceptions are very decided lateral jets, which appear to slope indifferently in both directions.

(5.) That faculæ should have a proper motion in the opposite direction to that of a spot.

(6.) That a spot should be more frequently bridged from the West, as we see the Sun, than from the East.

(7.) That there ought to be regions, even near the equator, relatively free from spots for long periods of time; for when disturbance has once begun, it will be long continued.

We do not know whether this could be shown from observation.

(8.) The period of spots ought to coincide with that of some assignable cause of disturbance of pressure.

Rugby, 1873, *Nov.* 11.

On the Determination of Time from Sextant Observations.
By William Lassell, Esq.

It is of late but seldom that I have the pleasure of sending to the Society a communication—indeed I have for some time

ceased to make regular observations with the two-foot equatorial, and what I now do in astronomy is of a very desultory and occasional kind. However, having nearly ceased to observe with a large instrument, I have taken, in this kind of way, to a very small one, and in another department of astronomy—namely, the determination of time.

Since I sent home the Sheepshanks thirty-inch transit instrument, the loan of which I had been permitted to enjoy for some years, I have felt sometimes at a loss for the means of regulating and ascertaining the errors of my clocks—though I had no especial reason for ascertaining their errors within a very small quantity, such as a second or two. But I used many years ago to observe, for determination of time, and occasionally of latitude, with various sextants and circles from six to ten inches radius; and I always found the occupation an interesting one.

The only instrument of this kind I now possess is a small sextant, of only three inches radius, which I purchased from the late Mr. Dollond more than forty years ago. I think he said he had made it for Mr. Davies Gilbert, who for some cause or other had not retained it. The sextant packs in a square box 4·25 inches in the side, and 2·4 inches deep, inside measure. It has a ball-and-socket joint, affixed to a sliding tube, which being screwed into the lid of the box, forms a stand. The telescope, at focus, is 3·4 inches long; and, though it has a clear aperture of only four tenths of an inch, it has two powers, magnifying respectively six and eleven times, defining admirably with the latter, which has quite sufficient light for stars of the second magnitude. The box also contained, when I received it, what professed to be an artificial horizon, being a circular disk about two inches in diameter, of parallel plate-glass, placed in a boxwood cup, to be filled with mercury, the glass disk intended to float horizontally and centrally upon it. But the disk never would remain central; and, though Mr. Dollond introduced four equidistant needle-points to keep it in the centre, it would always be in contact with one or other of the needle-points, thus vitiating its horizontality. I therefore very soon laid it aside.

I afterwards cast and polished a plane speculum, of about the same diameter, mounting it in a brass ring, supported by three equidistant adjusting screws with very fine threads. For levelling this I applied again to Mr. Dollond, and he furnished me with a glass level, the tube about three inches long, and, to quote the inscription upon it—" Ground parallel to show seconds ;" which means that one part of its circumference was ground flat throughout its whole length, and parallel to a tangent to the middle of the internal upper surface of the tube; so that, when placed upon a horizontal surface, the bubble should be in the middle. I fared no better with this. The level could never be taken up and replaced with a certainty of the same indication; and reversing might go on for ever without arriving at a uniform or

consistent result. It became tiresome beyond endurance. The reason seemed to be—either that the ground flat surface of the level was not truly plane; or, invisible particles of dust settled on the speculum, and became attached to the level, or otherwise prevented its coming into true contact with the speculum. That the latter was not in fault was proved by a critical scrutiny of objects reflected in it with a telescope of considerable power.

I then ground some levels myself, and selected one having a scale of 5° of inclination, equal to a displacement of the bubble of two hundredths of an inch, which I mounted in a brass tube with three points or feet. This has proved permanently satisfactory, making the sextant thus complete as a travelling companion. After all, if a vessel of mercury is to be had, and no vibration forbids its use, I always employ it, preferring it to any levelled horizon whatever.

But what has induced me to venture to trouble the Society with all this detail, is that I have found much greater accuracy attainable with these small means than I had expected; and I think it possible that some Fellows of the Society, familiar with this kind of observation, may not be unwilling to hear what can be done with so small an instrument. It may be, however, that others have done as much before, without its having come under my notice; and I merely relate what I confess has excited my surprise.

I proceed now to extract a few observations from my journal.

1873, July 19.—Altitudes for time taken with three-inch sextant, power 11, mercurial horizon, Dollond roof. Times by sidereal watch (= S W) compared with sidereal clock (= S):—

	h m s		s
At	17 19 45	S W – S =	7·0
	19 43 35		5·0
	20 58 12		4·0

Arcturus. 1st mag. West. — α Androm. 2nd mag. East. Without roof.

Times. h m s	Altitudes. ° ′ ″	Results. s	Times. h m s	Altitudes. ° ′ ″	Results. s
18 54 54·4	54 4 30	S. fast 0·5	20 28 22·8	88 41 0	S slow 1·5
58 48·8	52 51 30	0·1	31 20·0	89 36 0	3·8
19 0 32·8	52 19 30	0·7	33 16·4	90 10 30	fast 1·4
2 46·0	51 38 0	0·4	35 19·2	90 48 30	slow 1·7
5 0·0	50 56 0	slow 0·3	37 21·6	91 25 45	1·3
7 19·6	50 12 0	2·7	41 35·6	92 43 0	0·9
	Mean = S slow	0·22		Mean = S slow	1·81

Mean of both sets = S slow $1^s{\cdot}01$.

Index error accounted zero. Approximate position of Ray Lodge, latitude 51° 31′ 23″, longitude $2^m\ 49^s.3$ west.

1873, July 28.—Same instrument, mercurial horizon and roof. Times by sidereal watch:—

	h m s		s
S=	17 59 43	S−S W=	43·0
	19 40 52		43·6
	21 13 52		44·4

α Coronæ. 2nd mag. West.

Times. h m s	Altitudes. ° ′ ″		Results. s
20 3 59·6	67 37 30	=S slow	22·0
10 44·2	65 32 15		21·0
13 36·8	64 39 0		20·1
15 48·4	63 59 0		17·6
17 54·0	63 19 30		19·7
20 30·4	62 31 0		19·8
22 20·4	61 57 0		19·8
		Mean =S slow	20·0

α Androm. 2nd mag. East.

Times. h m s	Altitudes. ° ′ ″		Results. s
20 40 33·2	92 46 0	=S slow	24·9
43 8·0	93 31 15		19·2
45 33·6	94 16 30		23·0
49 59·6	95 36 30		22·2
52 27·6	96 20 30		20·5
		Mean =S slow	21·96

Mean of both sets = S slow $20^s.98$.

August 21.—Altitudes as before for time. Mercurial horizon without roof. Times by sidereal watch. S − S W = $1^m\ 59^s.5$:—

α Androm. 2nd mag. East.

Times. h m s	Altitudes. ° ′ ″		Results. s
20 36 20·8	91 48 30	=S slow	12·9
39 5·6	92 39 0		14·1
41 20·8	93 20 0		13·9
43 55·6	94 7 0		14·3
		Mean =S slow	13·80

α Coronæ. 2nd mag. West.

Times. h m s	Altitudes. ° ′ ″		Results. s
21 3 29·2	49 0 0	=S slow	13·6
6 38·8	48 2 30		14·5
8 54·8	47 22 0		13·5
10 44·4	46 49 30		11·9
13 21·2	46 1 30		15·1
		Mean =S slow	13·72

Mean of both sets = S slow $13^s.76$.

Sept. 1.—Same instrument. Mercurial horizon without roof:—

	h m s		s
S=	20 54 34	S W−S=	42·0
	22 1 38		42·0
	23 52 52		43·6

Index error = + 20″

α Coronæ. 2nd mag. West.

Times. h m s	Altitudes. ° ′ ″	Results. s
21 15 25·6	46 7 0=S slow	32·7
19 48·8	44 48 0	33·1
22 30·0	44 0 0	33·3
24 36·4	43 22 15	33·5
26 51·2	42 42 30	32·7
	Mean=S slow	33·06

Capella. 1st mag. East.

Times. h m s	Altitudes. ° ′ ″	Results. s
22 44 57·6	62 37 30=S slow	33·9
51 51·6	64 22 30	34·2
54 17·6	65 0 0	34·9
56 45·6	65 38 0	34·9
23 0 0·0	66 28 0	34·5
	Mean=S slow	34·48

Mean of both sets=S slow 33·77.

α Arietis. 2nd mag. East.

Times. h m s	Altitudes. ° ′ ″	Results. s
22 18 45·6	78 8 30=S slow	33·5
24 48·0	79 58 30	34·4
27 44·0	80 50 0 windy	29·2
	Mean=S slow	33·37

ζ Herculis. 3·2 mag. West.

Times. h m s	Altitudes. ° ′ ″	Results. s
21 32 48·0	67 57 0=S slow	28·3
35 33·6	67 6 15	29·0
38 15·2	66 16 45	29·4
40 58·0	65 26 30	31·3
43 38·0	64 38 30	29·1
	Mean=S slow	29·42

Mean of both sets=S slow 31ˢ·40.

The last two sets of altitudes I should not depend much upon; and they are introduced principally to show the probable error under unfavourable circumstances, ζ Herculis not being bright enough, in the then state of the sky, and the altitudes of α Arietis being too few, and those disturbed by wind. Also a reason for rejecting α Arietis is the danger, without extreme care, of making α and β to osculate.

Sept. 22.—Mercurial horizon without roof. Sky hazy—increasingly so—so that at the close Capella was too dim to enable me to determine the index error. The index error, however, of this sextant is very small and very constant. Times by sidereal watch:

	h m s		s
S=	22 22 44	S W−S=	44·0
	23 56 47		45·4

α Lyræ. 1st mag. West.

Times. h m s	Altitudes. ° ′ ″	Results. s
22 46 50·0	89 57 0=S slow	11·8
50 22·8	88 52 30	9·8
54 10·4	87 41 45	13·6
56 41·6	86 56 30	10·6
59 36·0	86 2 0	14·7
23 2 25·2	85 11 0	13·4
	Mean=S slow	12·32

Capella. 1st mag. East.

Times. h m s	Altitudes. ° ′ ″	Results. s
23 18 54·8	71 19 30=S slow	12·6
22 23·2	72 16 0	15·9
26 52·0	73 27 0	11·8
29 28·4	74 10 0	15·2
33 1·2	75 7 30	14·7
36 21·2	76 1 30	13·4
	Mean=S slow	13·93

Mean of both sets = S slow 13ˢ·12.

The discrepancies of the individual altitudes will not seem extravagant when it is remembered that Capella, for instance, while these were taken, was rising about 16″ of altitude in one second of time, the sextant only reading to 30″; and I may add to this my deteriorated vision, by which I am often sorely puzzled to read off the sextant in any degree to my satisfaction. I think it unnecessary to add that there are *no altitudes rejected*, the whole series being given; and that the whole of the observations are, I believe, as far from being 'cooked' as is possible. It is my practice to work out each altitude separately; as, if grouped together and the mean of the altitudes and times taken, a blunder in one observation would be masked and might vitiate the whole. The altitudes of this evening, thus treated, give for α Lyræ S. slow $12^s{\cdot}0$, and for Capella $15^s{\cdot}3$ mean $= 13^s{\cdot}6$, agreeing within about half a second with the mean of the individual results. When there is a perceptible breeze I do not hesitate to use the Dollond roof, generally taking half the number of altitudes with the roof in a given position, and reversing it for the other half. I do not find, however, any sensible error traceable to the roof. Altitudes of the Sun I very rarely take; for, although it is a more agreeable and easier operation, and the sextant is more accurately read by sunlight than lamplight, there is the disadvantage of not having any other heavenly body whose altitude can be taken at about the same time, to balance or correct the solar observation.

For keeping and carrying on my time, when I have got it, I generally use a mural transit, being a telescope firmly braced to a thick wall, and directed approximately to the equator, and within a few seconds of the meridian; its exact deviation from which having been originally determined and occasionally verified by sextant observations. The object-glass is an excellent one of 2·20 inches aperture and 25·8 inches focus, the eye-piece magnifying 32·6 times. It has three rather stout wires, about 40 seconds apart, and is furnished with the means of illumination. The field of view is large, and, by means of a positive eye-piece sliding vertically, it embraces all stars between about 88° 15′ and 91° 45′ polar distance. My observing list includes about 170 stars, not lower than the seventh magnitude of the British Association Catalogue. Of this magnitude, however, only about 27 are introduced in parts of the sky rather bare of larger stars, and these are abundantly bright even in a somewhat hazy and dull sky.

The annual precessions are stated in my transit-list, and the corrections of R.A. due to the season of the year are obtained by inspection from a short supplemental table. Of course I do not pretend thus to ensure the extremest accuracy, my requirements generally being satisfied with the belief or conviction that the probable error of any determination does not exceed one second. The large number of stars passing ensures, even on a fitful night, frequent opportunities of observation.

This is simply another disc on the same spindle, the reverse of the disc $\gamma\ \delta\ \varepsilon\ \sigma$—that is to say, entirely insulated on the edge *except* at the points γ and ε, which we shall call $\gamma'\ \varepsilon'$.

There is a spring like K, which we shall call K′, pressing on this disc, and when the instantaneous current takes place, when K′ is at γ' it is made to pass through an electro-magnet *c c c*, and from the drawing (Plate III.), the manner in which the switch is brought to the centre will be evident.

The whole apparatus may appear somewhat complicated from this description, though, indeed, it is not so.

The power of control depends on the weight of the bars *a k n* and *b l m*, and the character of the friction surfaces, and can be almost indefinitely regulated, though, of course, when pushed too far, at the expense of uniformity of motion. It remains to be considered how far uniformity of motion is sacrificed by this control. All control must consist of oscillation, and all we can do is to reduce that oscillation to a harmless amount.

We have found from experiment that *without control* no change of work which the clock can be called upon to perform, that is, from driving only itself to driving the telescope, and that considerably and disadvantageously out of balance, affects the clock rate $\frac{1}{100}$ part, therefore the clock can commit no error in one second so great as the $\frac{1}{100}$ second.

We have adjusted the extreme limits of control to correspond to one second per minute; that is, that the clock may err $\frac{1}{120}$ second on either side of the point of sympathy in a second, and would be corrected during the succeeding second.

The effect is to produce a control so perfect that *no variation whatever* can be perceived on the second hand of the driving clock from that of the standard clock, during any number of hours, and no sensible oscillation of a star relative to the micrometer web under high powers.

It is no exaggeration to say that when the clock is driving the telescope the observer may add his own weight to the driving weight without affecting the accuracy of control.

The construction of the disc $\gamma\ \delta\ \varepsilon\ \sigma$, we have just described, permits the use of instantaneous currents at half-second intervals, and thus the power of control can be doubled, or the oscillation halved at pleasure. We have found the currents from a seconds pendulum however so entirely satisfactory that we have not as yet employed a half seconds pendulum.

The clock is the workmanship of Messrs. T. Cooke and Sons, York, and is most highly creditable to their firm.

We must conclude by expressing our obligations to them for the trouble they have taken, and the intelligent manner in which they have carried out our designs.

The Observatory, Dunecht.

Note on the Star 515 *in Oeltzen's Catalogue of Schwerd's Stars.* By R. C. Carrington, F.R.S.

It will be recollected that at page 150 of my Catalogue of Circumpolar Stars, I added this note:—

"There is one star in Oeltzen's Catalogue, No. 515, of the 7th magnitude, observed once only by Schwerd, the position of which for 1855·0 is

$$\text{R.A.} = 8^{h}\ 21^{m}\ 43^{s}{\cdot}1. \qquad \text{N.P.D.} = 6^{\circ}\ 46'\ 58''{\cdot}1.$$

which I have no observation of, and do not find to exist. Mr. Oeltzen finds no error in his reduction on re-examination."

I have frequently referred to this place in the heavens, and have found no trace of the star, and it became an object with me to find what error Schwerd had made in his Catalogue. The place stands thus in the Catalogue:

1826, Oct. 19.

No.		Mag.	1	2	3	8	4 (h m s)	9 (m s)	5 (s)	6	7
753	S.P.	7	—	—	—	—	20 15 43:	16 32·0	24·5	—	—
			Nonien.		I.	III.	II. IV.	Niveau.	Fad.	Decln.	
			222° 38′		5″	5″	— —	−10″·9 + 10″·2	9	83° 18′	

Apparently all correct. But it must be noted that 83° 18′ is not observed, but deduced from 222° 38′; and $20^{h}\ 15^{m}\ 43^{s}$: is the mean wire deduced also from 83° 18′, on the supposition that the minute of wire 5 (3 really) is 18^{m}. Now it struck me to try 17^{m} as the minute, and to see what change that would make in the reduction, and I found it would require a change of 8° in the declination. Accordingly I supposed 214° 38·5″ | 5″ and 75° 18′ to be the declination of the star, and then found

h m s		
8 16 7·17	for mean wire	
8 16 24·60	for App. R.A.	
8 16 32·71	for R.A. 1828·0	$= 8^{h}\ 19^{m}\ 49^{s}{\cdot}88$ for 1855·0
° ′ ″		
75 19 2·2	observed decln.	
— 1 24·2	refraction	
75 17 38·0	for App. Dec.	
75 17 47·7	for Dec. 1828·0	$= 75^{\circ}\ 12'\ 40''{\cdot}7$ for 1855·0

which shows that the star 515 of Oeltzen's Catalogue is a previous observation, under disguise, of his next star 516.

They are both found in Argelander's Zone, 75°, under No. 342, of 6·4 magnitude and $8^{h}\ 19^{m}\ 48^{s}$—75° 13′·2—for 1855·0.

There can be no doubt that 214° should be the circle reading, and that Schwerd did not put it down at the time.

Observations of the Periodical Comets of Tempel and Brorsen. By George Bishop, Esq.

I beg to communicate to the Society the following positions of the periodical comet of *Tempel* (discovered July 3) and of *Brorsen's* comet of short period, obtained in this observatory. The observations have been chiefly made by Mr. Plummer. Three by Mr. Hind are distinguished by H.

Tempel's Comet.

	Twickenham Mean Time. h m s	Apparent R.A. h m s	Apparent Decl. ° ′ ″	Observer.
July 31	13 24 33	1 18 44·46	−7 54 4·3	P
Aug. 2	13 2 58	1 22 31 59	8 15 17·1	H
„ 6	14 21 22	1 29 36·92	9 3 11·6	H
Sept. 17	13 52 22	1 49 47·13	−18 21 11·5	P
„ 20	13 51 57	* — 53·14	*— 3 28·3	„
„ 21	14 5 0	* — 46·46	*—10 25·8	„
„ 26	13 41 7	1 43 13·29	−19·37 20·5	„
Oct. 20	11 35 17	1 22 10·1	19 55 41	„
„ 20	11 39 49	1 22 11·4	−19 55 59	H

The stars with which the comet was compared on the nights of the 20th and 21st September are not found in any catalogue, and on the 20th October, owing to the extreme faintness of the comet, the observers placed no reliance on their observations. For this night the position is given approximately.

Brorsen's Comet.

	Twickenham Mean Time. h m s	Apparent R.A. h m s	Apparent Decl. ° ′ ″
Sept. 17	15 45 39	8 50 7·44	+ 3 23 45·6
„ 21	16 8 5	9 17 24·24	4 36 40·1
Oct. 8	16 32 26	11 13 2·66	7 37 17·7
„ 15	18 9 25	11 59 19·11	7 21 0·4
„ 26	17 16 18	13 2 22·09	+ 5 30 26·8

Oct. 8. The comet observed through fog.

„ 15. The observation rendered doubtful owing to fog and the near approach of daylight.

Oct. 26. The comet's diameter estimated at 1¼ minutes. The comet round, with considerable condensation towards the centre.

The Observatory, Twickenham,
1873, *Nov.* 12.

Parabolic Elements of the Comets of Henry (Paris) and Borrelly (Marseilles). By Mr. W. E. Plummer.

(*Communicated by George Bishop, Esq.*)

The following elements of Comet IV. 1873, discovered at Paris by Paul Henry on August 23, are computed from obser-

vations made at Washington on August 25 and September 2, and at Twickenham on September 12. Corrections for parallax and aberration have been applied, computed from the orbit of Mr. Ormond Stone.

Perihelion Passage. Oct. 1·76295 G.M.T.

		° ′ ″	
π	...	302 58 51·0	M. Eq. Sept. 0.
☊	...	176 43 35·4	
i	...	58 30 40·1	
Log. q	...	9·5852771	

Motion Retrograde.

A comparison with the middle place shows the following errors in the sense, calculation-observation.

Longitude $-1''\!\cdot\!8$. Latitude $-1''\!\cdot\!6$.

The orbit of Comet ~~III~~ IV. 1873, discovered by M. Borrelly, is computed from observations made at Marseilles on August 23, at Königsberg on September 2, and at Twickenham on September 17, corrected for parallax and aberration deduced from elements computed by myself, but embracing a shorter interval, and to which the following are preferable.

Perihelion Passage. Sept. 10·73619 G.M.T.

		° ′ ″	
π	...	36 57 3·7	M. Eq. Sept. 0.
☊	...	230 38 37·2	
i	...	84 3 20·1	
Log. q	...	9·9002537	

Motion Retrograde.

The errors for the middle observation are—

$$\text{C}-\text{O} \quad \delta\lambda\cos\beta \quad -15''\!\cdot\!8$$
$$\delta\beta \quad -6\cdot2$$

Mr. Bishop's Observatory, Twickenham,
1873, *November* 12.

Observations of Comets and Minor Planets made at the Observatory of Marseilles.

(*Communicated by M. Stéphan through Mr. Hind.*)

Comet II. 1873 (*Tempel*).

1873	Mean Time Marseilles. h m s	R.A. h m s	Log. fac. par.	N.P.D. ° ′ ″	Log. fac. par.	Comp. Star.	Obs.
July 29	13 57 39	1 14 46·05	−9·463	97 32 19·1	−0·8300	*a*	B
30	14 13 32	1 16 48·28	−9·422	97 43 9·5	−0·8293	*a*	S
31	15 3 32	1 18 50·66	−9·252	97 54 22·4	−0·8369	*b*	C
Aug. 30	13 55 31	1 52 55·99	−9·187	104 32 57·9	−0·8721	*c*	B
Sept. 18	12 28 51	1 49 13·36	−9·237	108 30 38·3	−0·8869	*d*	B
19	11 42 53	1 48 36·01	−9·387	108 40 1·2	−0·8776	*d*	B
24	13 44 51	1 44 54·49	+8·498	109 23 48·6	−0·8987	*e*	C

Comet III. 1873 (*Borrelly*).

Aug. 20	15 43 40	7 27 1·57	−9·744	51 15 46·5	−0·6509	*f*	B
21	14 54 14	7 28 20·02	−9·736	52 13 26·5	−0·6299	*g*	S
26	15 41 49	7 35 33·60	−9·708	57 45 29·2	−0·6814	*h*	B
30	14 58 43	7 41 56·44	−9·689	62 51 19·6	−0·7287	*i*	B
Sept. 1	14 40 51	7 45 5·65	−9·677	65 41 5·7	−0·7277	*k*	B
16	16 23 32	8 21 31·77	−9·594	93 48 32·2	−0·7976	*l*	B
18	16 29 45	8 27 51·59	−9·586	98 16 16·0	−0·8118	*m*	B

Comet IV. 1873 (*Henry*).

Aug. 24	12 48 24	7 33 24·77	−9·925	30 44 51·9	−0·3712	*n*	S

Comet V. 1873 (*Brorsen*).

Sept. 20	15 19 2	9 10 26·31	...	85 40 49·2	...	*o*	S
24	16 59 6	9 38 1·99	...	84 32 51·0	...	*p*	C

Comet VI. 1873 (*Faye*).

Sept. 3	16 9 3	7 0 48·14	...	74 12 35·2	...	*q*	S

The observations of Comets V. and VI. are corrected for the effect of parallax.

Mean Positions of the Comparison-Stars for 1873·0.

		Name of Star.	Mag.	Mean R.A. h m s	Mean N.P.D. ° ′ ″	Authority.
Comet II. 1873	*a*	W. B. (1) I. 212	9	1 14 37·70	97 37 27·6	Weisse's Bessel.
„	*b*	W. B. (1) I. 271	7	1 17 57·38	97 34 42·7	„
„	*c*	W. B. (1) I. 930	7	1 53 21·56	104 29 27·1	„
„	*d*	Lalande 3631	8	1 51 11·75	108 17 39·0	Lalande.
„	*e*	Lalande 3339	9	1 42 45·89	109 0 53·9	„
Comet III. 1873	*f*	W. B. (2) VII. 887	6	7 31 41·82	51 22 1·6	Weis. Bes.
„	*g*	W. B. (2) VII. 684	8	7 24 55·79	52 24 14·6	„
„	*h*	W. B. (2) VII. 996	9	7 34 48·68	57 41 26·6	„
„	*i*	B. A. C. 2617	5	7 45 43·46	62 54 25·9	B. A. C.
„	*k*	W. B. (2) VII. 1206	7	7 43 1·16	65 31 17·2	Weis. Bes.
„	*l*	B. A. C. 2831	6	8 20 6·41	93 34 16·2	B. A. C.
„	*m*	W. B. (1) VIII. 804	7	8 31 39·49	98 26 16·7	Weis. Bes.
Comet IV. 1873	*n*	Oeltz. Arg. 8087	8–9	7 29 37·07	30 37 0·4	Oeltz. Arg.
Comet V. 1873	*o*	W. B. (1) IX. 102	8	9 6 46·52	85 39 15·7	Weis. Bes.
„	*p*	Lalande 19072	9	9 36 13·42	84 23 44·0	Lalande.
Comet VI. 1873	*q*	W. B. (2) VII. 11	9	7 3 4·46	74 11 18·2	Weis. Bes.

Observations of Minor Planets (133) and Sophrosyne (134).

(133) *Watson.*

	Mean Time Marseilles. h m s	R.A. h m s	Log. fac. par.	N.P.D. ° ′ ″	Log. fac. par.	Comp. Star.	Obs.
Aug. 19	14 10 39	23 0 38·20	+9·079	92 43 30·6	−0·8059	*a*	S
20	12 36 48	22 59 56·30	−8·682	92 45 30·7	−0·8069	*a*	S
21	13 29 15	22 59 6·67	+8·775	92 47 41·7	−0·8073	*b*	S
28	9 46 2	22 53 45·25	−9·445	93 5 0·7	−0·8032	*c*	B
29	8 44 34	22 52 58·83	−9·547	93 7 34·1	−0·7995	*c*	B
30	9 13 55	22 52 9·12	−9·491	93 10 28·5	−0·8015	*c*	B
Sept. 1	9 14 31	22 50 31·17	−9·472	93 16 11·3	−0·8039	*c*	B
19	9 11 46	22 36 35·84	−9·220	94 9 32·2	−0·8147	*d*	B
24	9 36 26	22 33 19·80	−8·898	94 23 58·4	−0·8182	*e*	C
25	9 56 6	22 32 42·64	−8·619	94 26 45·4	−0·8190	*e*	C
27	9 46 4	22 31 33·30	−8·536	94 31 32·9	−0·8194	*e*	C
28	9 56 8	22 30 59·36	...	94 33 54·1	−0·8199	*e*	C
29	10 7 10	22 30 26·73	+8·358	94 36 18·0	−0·8201	*e*	C

Name of Star.	No. of Obs.	R. A. 1870. Oxford.	W.	O—W.	No. of Obs.	Decl. 1870. Oxford.	W.	O—W.
		h m s	s	s		° ′ ″	″	″
α Ophiuchi	6	17 28 54·07	54·05	+0·02	10	+12 39 23·21	25·41	−2·20
[μ Herculis]	7	17 41 22·24	22·26	−0·02	7	+27 47 54·82	56·25	−1·43
γ Draconis	4	17 53 35·06	35·40	−0·34	10	+51 30 18·04	18·40	−0·36
α Lyræ	8	18 32 32·16	32·25	−0·09	15	+38 39 50·39	51·32	−0·93
[β¹ Lyræ]	7	18 45 16·81	16·83	−0·02	13	+33 12 47·74	47·94	−0·20
[δ Aquilæ]	6	19 18 56·53	56·60	−0·07	7	+ 2 51 27·32	28·40	−1·08
γ Aquilæ	2	19 40 4·74	4·79	−0·05	3	+10 17 53·13	54·54	−1·41
α Aquilæ	12	19 44 26·38	26·47	−0·09	15	+ 8 31 36·27	37·35	−1·08
β Aquilæ	8	19 48 55·65	55·70	−0·05	6	+ 6 5 1·49	2·31	−0·82
α² Capric.	2	20 10 50·38	50·44	−0·06	1	−12 56 44·68	44·85	+0·17
α Cygni	6	20 36 59·94	60·03	−0·09	14	+44 49 0·07	0·95	−0·88
α Cephei	7	21 15 28·37	28·52	−0·15	30	+62 2 7·22	5·96	+1·26
β Cephei	4	21 26 58·28	58·38	−0·10	27	+69 59 24·46	23·69	+0·77
α Aquarii	6	21 59 6·34	6·41	−0·07	5	− 0 57 1·31	0·86	−0·45
α Pisc. austr.	1	22 50 27·69	27·75	−0·06	1	−30 18 39·11	37·91	−1.20
α Pegasi	7	22 58 17·17	17·23	−0·06	6	+14 30 22·16	24·02	−1·76
[γ Piscium]	9	23 10 25·52	25·56	−0·04	10	+ 2 34 20·61	20·[illegible]6	+0·05
[ι Piscium]	2	23 32 15·77	15·99	−0·22	2	+ 4 55 16·68	18·55	+1·87
[ω Piscium]	4	23 52 38·14	38·25	−0·11	5	+ 6 8 37·55	37·08	+0·47
α Urs. Min.	71	1 11 16·84	17·49	−0·65	104	+88 [illegible]6 58·35	59·10	−0·75
δ Urs. Min.	10	18 14 16·13	16·95	−0·82	60	+86 36 20·79	20·33	+0·46

Note by Mr. Drach on the Ancient Rabbinical Cubit-measure.

Mr. Drach remarks on Prof. Wackerbath's Ancient Rabbinical cubit-measure (*Monthly Notices*, Supp. Number, 1873, vol. xxxiii.), that the *Zohar* numbers 6,000 and 12,000, may be partly derived from the Assyrian *Saros*, or the Jewish millennium of 6,000 years, and partly from the Alexandrian Schools. The *Imri Binah* was written by Issachar Baer ben Moses Pethaiah (Prague, 1610). Mr. Drach showed in *Trans. Soc. Biblical Archæology*, vol. i. p. 336, that the ancient Egyptians might have taken the *circumference* of the Earth as an *integral nmmber* of units, and not the inaccessible diameter according to Prof. Piazzi Smyth's Pyramidal notions; thus falling into the same groove as the French metrologues of the eighteeenth century.

1873, *November* 14.

Discovery and Elements of Minor Planet (134)

This planet was discovered on the evening of September 27 by Dr. R. Luther, and was of the 10·11 magnitude. The following observations were made at the Observatory of Bilk on the night of the discovery and on the following evening.

1873	M.T. Bilk. h m s	R.A. (134). h m s	N.P.D. (134). ° ′ ″	
Sept. 27	9 38 37·3	0 8 22·74	82 6 17·9	10 comp.
27	11 24 22·0	0 8 17·26	82 6 22·6	10 ,,
28	9 27 1·6	0 7 20·36	82 7 14·6	10 ,,

At the request of Dr. Luther, Professor Argelander and M. Theodor Wolff, of Bonn, have named this planet *Sophrosyne.*

The following elements have been computed by Dr. Tietjen:—

1873, Oct. 13·5 M.T. Berlin.

		°	′	″	
M	=	314	37	42·3	
ω	=	80	45	3·2	Mean Equinox 1873·0.
☊	=	346	7	5·5	Mean Equinox 1873·0.
i	=	11	22	21·8	Mean Equinox 1873·0.
ϕ	=	7	22	29·1	
μ	=		862″·7408		
log. a	=		0·409417		

Discovery of Comet VII. 1873.

This Comet was discovered by M. Coggia, at Marseilles, on November 10, and by Dr. Winnecke, at Strasburg, on November 11. M. Coggia remarks that it was, when first seen, of extreme faintness. The following approximate elements have been computed by Prof. E. Weiss from observations made at Strasburg on November 11, and at Vienna on November 12 and 13:—

Perihl.Pass. 1873, December 4·1348 Berlin Mean Time.

		°	′	″	
π	=	94	23	14	Mean Equinox 1873·0
☊	=	254	14	9	Mean Equinox 1873·0
i	=	27	2	7	Mean Equinox 1873·0
log. q	=	9·83810			

Professor Weiss remarks that the orbit of this comet resembles that of the first comet of 1818, discovered by Pons at Marseilles. Later observations seem to confirm this resemblance.

Mr. Hind has also calculated the following elements from the observations at Marseilles on November 11, and at Hamburg on November 12 and 13:—

Perih. Pass. 1873, November 30·2784 G.M.T.

	°	′	″	
π	83	12	15	Appar. Eq. 1873, Nov. 12.
☊	249	23	2	
i	30	44	32	
log. q	9·873503			

Motion direct.

Mr. Hind also remarks that this orbit has a general resemblance to that recently given by him in the *Monthly Notices* for Pons' comet of February, 1818 (vol. xxxiii. p. 50).

Observation of a Solar Spot. By C. L. Prince, Esq.

It may be worth recording that, on September 18, I observed, on the N.P. quadrant of the Sun, a small black spot of about five seconds in diameter. I have no recollection of having seen a solar spot so nearly circular and almost entirely devoid of penumbra. In a telescope of not more than two inches aperture it might have been taken for a planetary disc.

The Observatory, Crowborough Beacon,
1873, *October* 31.

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

Vol. XXXIV. *December* 12, 1873. No. 2.

Professor Cayley, F.R.S., President, in the Chair.

John Berger Spence, Esq., Erlington Hall, Manchester;
Dr. Robert Brown, Birkenhead College;
Prof. W. K. Clifford, M.A., University College, London;
Josh. Hough, Esq., B.A., Rossall School, Fleetwood; and
John Thos. Seccombe, Esq., M.D., King's Lynn,

were balloted for and duly elected Fellows of the Society.

The New Savilian Observatory for Astronomical Physics at Oxford.

The establishment of an Observatory for Astronomical Physics in one of our most ancient seats of learning, cannot fail to be a matter of great interest to the Members of the Royal Astronomical Society. Under this impression it is proposed to give some account of the circumstances which ultimately led to the undertaking, and of some of the objects which it is hoped will be realized thereby.

The Savilian Professor of Astronomy in Oxford, shortly after his appointment, conceived it to be his duty to lay before the proper authorities in the University, the importance of being furnished with instrumental means adequate to the instruction of his class, and for the purposes of original research. The formal application was made to Convocation in March of the present year; when the University, with great, but by no means unusual liberality, voted a sum of money sufficient for the purchase of a Refracting Telescope of 12½-inches aperture, and for providing a suitable building to contain it. It was contemplated

D

that this great telescope should be furnished with every appliance which experience had proved to be conducive to the requirements of modern research, and to the saving of physical labour in the use of the instrument. These conditions Mr. Grubb of Dublin has undertaken to fulfil in the terms of his contract with the University; and, judging from what that artist has already performed, no doubt need be entertained that all proper expectations will be realized.

Within the official lifetime of the present Astronomer Royal, an aperture of even* five inches for a refracting telescope, was a circumstance of extreme rareness, if not utterly unknown; such, however, has been the advance both in the manufacture of optical glass, and in the construction of telescopes, that no delay or difficulties were apprehended in procuring an instrument of the comparatively large dimensions referred to.

The Professor and those with whom he acted felt, at the time, that by the acquisition of this noble instrument, at all events the foundation was laid for eventual astronomical arrangements in the University of a far more extensive nature, but they resolved to wait patiently for some suitable, though it might be distant, opportunity for again pressing the claims of this particular branch of science. Had this been all that had transpired relating to the progress of Astronomy in Oxford, probably it would not have been thought desirable to trouble the Royal Astronomical Society with any formal account beyond a brief and passing notice of the liberal provision which the University had thus made for the scientific advantage of its students.

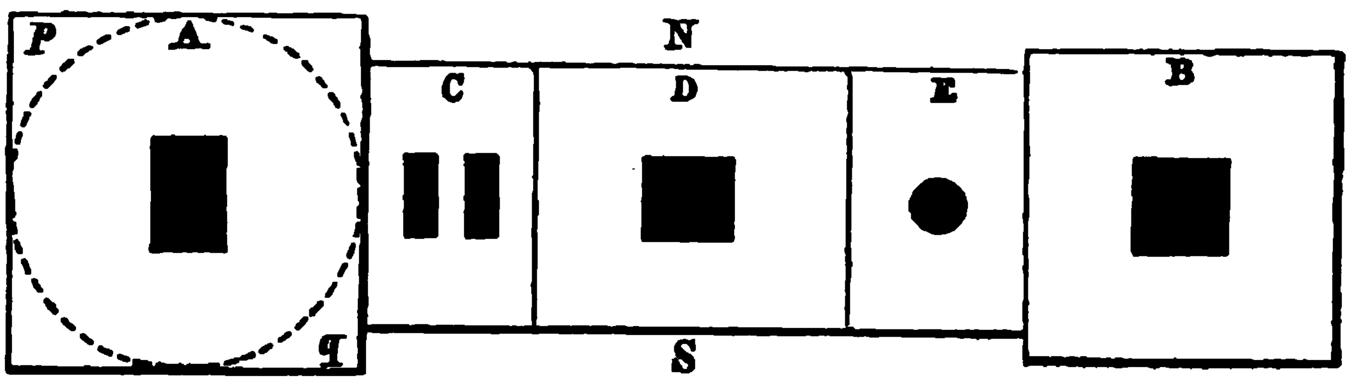
Very shortly, however, after the above arrangements had been completed, Dr. Warren De La Rue, on hearing of the generous disposition of Oxford towards a science which had so successfully occupied a large portion of his life, offered to present the University with his far-famed Reflecting Telescope and the greater part of the contents of his Observatory, on the sole condition that they should be usefully employed. Unfortunately, it was the failure of his eyesight which was the proximate cause of our late President's desire thus to transfer his instruments from Cranford to Oxford: fortunately, this transfer could not be effected before their owner had made, through many years, the noblest use of instruments, which, as he says, "were constructed from his own drawings, and by the hands of his own engineers." There was also a certain gracefulness in thus offering to Oxford a magnificent telescope, for the scientific application of which, the University had recently conferred upon him the high distinction of an Honorary Doctor's degree.

Here, then, there seemed to be presented a fitting opportunity for at once establishing at Oxford a complete Observatory for

* There is a very interesting account of the difficulties which up to 1826 beset the construction of object-glasses beyond the aperture of four inches, in the *Memoirs* of the Royal Astronomical Society, vol. 2, page 507.

the new science of Astronomical Physics, which, under other circumstances, might not have been realised for years. A Committee of Scientific Professors was appointed for the consideration of the subject; and after much and most careful consultation, they recommended the University to accept the munificent gift, and to provide the pecuniary means both for housing the instruments, and for skilled assistance for the Professor in the use of them. The University, acting in the same spirit of liberality which has for many years characterised their dealings towards science, and notwithstanding their grant of £2,500 in March, again added in the month of November a still further sum, adequate for the purposes described.

The annexed diagram, though not intended to represent the architectural features of the building, will sufficiently well explain its general plan.



At A is a square tower, with rustic abutments at the angles, each side being 21 ft. 6 in. clear, inside. It consists of three floors. First, a basement floor, 3 ft. 6 in. below the level of the ground, and 9 ft. 6 in. high. There are windows north and south. Above this is a Computing Room, 12 ft. high. Thirdly, this is surmounted by the Equatoreal Room, destined to contain the great Refracting Telescope, now in course of construction by Mr. Grubb. The walls of this room are 8 ft. 6 in. high from the floor to the bottom of the shutter, and then comes the Revolving Dome, to be described hereafter.

The spaces *p* and *q* between the square walls of the Tower, and the cylindrical skirting of the Equatoreal Room, furnish convenient balconies for the observations of shooting stars and other phenomena in the open air. These balconies are approached from the Equatoreal Room through glazed doors, which will furnish sufficient light in the daytime. The pier for the telescope is built on a foundation of concrete, 9 ft. long, 7 ft. wide, 4 ft. 6 in. deep, and tapers off to 4 ft. 6 in. by 3 ft. This Tower will probably be designated the Savilian Tower. C, D, E, represent a corridor about 40 ft. long and 13 ft. wide. This corridor is to contain two instruments which have already been in use for the instruction of the Astronomical Class in the University. First, there is a basement floor, uninterrupted by divisions, and, similar to that of the Savilian Tower, 9 ft. 6 in. high. Above this is the floor for the instruments, on a level with the floor of the computing room of A. C will contain a Transit

Instrument, the aperture of which is 4 in., and focal length 5 ft.; of course there is also its clock. E will contain an Altazimuth Instrument, by Troughton and Simms, with 18-in. circles; it will be fixed in the meridian, and serve as a Transit Circle, for the instruction of the students. Each of the rooms C and E is 10 ft. long, 12 ft. high. D is a room 18 ft. long, and is intended to contain one of Dr. De La Rue's reflecting telescopes mounted in Altazimuth fashion: aperture 13 in., focal length 10 ft. This instrument will command about one hour on each side of the meridian, and uninterrupted sweeps from the North and South horizon to the zenith. It will be applied chiefly to Zone work; and inasmuch as its optical capacities are of the highest class, it will occasionally serve for the students, in the scrutiny of celestial objects. B is a tower similar in all respects to the Savilian Tower, except that the side of the square is 18 ft. 6 in., and the height of the walls of the Equatoreal Room is 3 ft. 6 in. instead of 8 ft. 6 in. The dome is similar to that of A. It is proposed to call this Tower the De La Rue Tower. In the basement will be placed Dr. De La Rue's polishing machine for large mirrors; it is sufficiently large for the construction of a mirror of 2-ft. aperture, and in due time will probably be set in requisition for the completion of mirrors of those dimensions, but of an unusually short focal length, perhaps 10 ft. There also will be placed, Foucault's apparatus for testing the qualities of mirrors and object-glasses. The basement also permits the admission of a beam of sunlight, nearly 80 ft. long; at a window on the east or west side may be placed a Heliostat, on a small stone balcony provided for the purpose. Most of the windows throughout the building are furnished with similar stone balconies, on which instruments may be placed. The first floor of the De La Rue Tower is subdivided into a small room for Photography and a room for the Professor. The Revolving Domes are peculiar in their construction. Inasmuch as by the great liberality of the University, permission has been given to place this Observatory in the most convenient position within their beautiful Park,* it was thought proper to consider the architectural character of the building, at any rate to the small extent permitted by the astro-

* The ground for this Park, consisting of about sixty acres, was purchased and laid out by the University, and then, with admirable liberality, thrown open to the public. The situation of the new Observatory is remarkably fortunate, and free from any injurious obstruction to the view in every direction. Nevertheless, it lies within an easy distance from and communicates with the University Museum, where are to be found the Lecture Rooms and Laboratories of the various scientific Professors. This Museum, again, is another instance of the munificence of the University, in providing at vast cost for the wants of her students in the natural sciences. Should the University see fit to assign, as is proposed, a portion of the Park contiguous to the new Observatory for the purposes of a Botanic Garden, then, this remarkable aggregation of appliances for every branch of Natural Philosophy, round a common centre, will form an arrangement, which for compactness and completeness has no present rival in the world: a monument and a pledge of the generous care of a true Alma Mater.

nomical exigencies of the case. With this view the form of a lofty, truncated Dome was selected for the revolving roofs, rather than the usual and convenient form of a drum. This necessitated difficult and unusual contrivances for the shutters. They are formed of revolving corrugated steel, extending from the bottom of the dome to about 2 ft. beyond the zenith. The domes revolve on a modification of a Smeaton's live-ring, consisting of twelve wheels. The framework of the dome is of light wrought-iron ribs, braced together with a lattice-work of strong wire; on this lattice-work strong canvas is quilted, and this is again covered with layers of felt and sail-cloth painted. External ribs will run down the roof from top to bottom. The domes are moved from the inside, by means of a large wheel having an India-rubber tire, pressed down on the kerb with any suitable pressure by means of a graduated spring, and turned round by an endless cord. There is also a special contrivance for keeping out the wind. The horizontal truncated top of the dome is surrounded by a light ornamental cresting or balustrade to conceal the box which contains the revolving steel shutters. The opening of the shutters is 3 ft. 6 in. wide.

It is part of the plan, that a Board of Visitors should be appointed for this Observatory, and their Report is annually to be laid before Convocation. This annual Report will, no doubt, find its way into the great Commonwealth of Science, and thus tend to awaken fresh sympathies, and to enhance the general respect for the University far beyond the precincts of Oxford.

No reference has, so far, been made to the Radcliffe Observatory, which has long occupied so eminent a position in the Astronomical Republic. The impression generally prevails that this Observatory is connected with and is under the control of the University of Oxford. Such, however, is not practically the case. The Radcliffe Observatory was founded, exactly a century ago, by the Trustees of the Radcliffe estates, who are not necessarily officers of the University. It originated at the instance of the Savilian Professor of Astronomy of that day, who failing to obtain astronomical instruments from the University, successfully applied for that purpose to the Radcliffe Trustees. The result of that wise and liberal disposition of their funds has been the establishment of an Observatory, which under its last two directors, Mr. Johnson and the Rev. R. Main, has, after Greenwich, proved itself to be second to none for the accuracy and value of its observations. The Savilian Professors are, by the Statutes of the University, no longer permitted to hold the office of Director of the Radcliffe Observatory. It is probable that the Radcliffe Observatory may continue in its sphere of usefulness within the limits of the old astronomy, while the new Observatory will strictly confine itself to the more recent branches of Astronomical Physics. Each may thus assist and illustrate the other. Without the hearty co-operation of Mr. Main, and indeed without the loyal and intelligent zeal, and unanimity of the other scientific

Professors, the establishment of the new Savilian Observatory would have been an impossibility in Oxford.*

C. PRITCHARD,
Savilian Professor of Astronomy.

Oxford, 1873, *December.*

* An Astronomical Library will be a necessity for the complete equipment of this new institution. The Professor will gratefully accept and acknowledge assistance therein from the various Observatories and learned Academies of Europe and America.

On the probable Variability of some of the Red Stars in Schjellerup's List, published in the Astronomische Nachrichten, No. 1591. By J. Birmingham, Esq.

(*Communicated by the Rev. T. W. Webb.*)

I find that the magnitudes of several stars in the above list now differ considerably from the magnitudes recorded, while, at least one star—No. 252—seems to have quite disappeared.

This star was twice observed by Sir John Herschel, and noted, in the Appendix to the *Cape Observations*, as "extremely intense ruby" and of the 8·5 magnitude. It does not appear, however, in the *Bonn Catalogue*.

My first search for it was in July 1872, when I failed to see it, and I was equally unsuccessful in several searches up to a recent date. In October of the present year (1873) I wrote to the Rev. Mr. Webb requesting him to look for it; but to him, and also to Mr. Lynn and other observers at the Royal Observatory, Greenwich, it was invisible. We are, therefore, presented with two hypotheses: either the star must be a variable, or Herschel committed a mistake in his observations. It may be remarked that there is a star near the place of 252, well agreeing with Herschel's description of the latter. The former is No. 251. It was observed by Bessel as red, and rated by him of the 8th magnitude. The positions of both for 1872 are as follows:—

	R. A.				Decl.		
	h	m	s		°	′	″
251 . . .	21	37	59;	+	37	25	50
252 . . .	21	39	4;	+	37	16	38;

showing a difference of over 1 minute in R.A. and 9′ in decl.

The grounds, therefore, for any suspicion of a mistake on the part of Herschel might be said to be, the proximity of the places of the two recorded stars, with their similarity of appearance as described, and the absence of 252 from the *Bonn Catalogue*.

I am not aware whether the red stars in the Appendix to the *Cape Observations* are to be considered as discoveries of Sir John

Herschel, or not. If he did not confine the list to his own stars, he would naturally include Bessel's very remarkable star in his observations; and, as he records only one red star in the locality, it would, on the foregoing supposition, add to the cause of suspecting that there was no second similar object about that place after all.

But how can we imagine that if Herschel's observations refer to Bessel's star, they would lead to results which must be considered very erroneous for an observer of his capability, particularly when he had Bessel's position available to check his work? And even if he had no such check to assist him, if he was ignorant (which is scarcely possible) of Bessel's observation, and viewed the star as a new one discovered by himself, still it would be most difficult to believe that he could commit such mistakes in his measurements. On the supposition that Herschel intended merely to give a list of his own red stars, there seems, in fact, to be no substantial ground for concluding that he did not see two pretty near each other, though he noted only one of them as a new discovery, and positioned it accordingly. The fact that neither Bessel nor Argelander seems to have noticed Herschel's star might be explained by the same variability that has caused its recent disappearance; but its invisibility to all observers except Herschel would seem to exclude it from the class of regular variables, as so many coincidences of minima would be unlikely to occur. It might, more probably, be one of those wonderful stars which have been observed only in a single brief period of extraordinary lustre. But whatever view may be taken of the matter, it is certain that no star can now be detected in the position of No. 252 in Schjellerup's Catalogue. I may add, that it is only one of many in that list that I have failed to find; but I bring it alone under notice as its non-appearance has been confirmed by observers about whose accuracy there can be no question.

I have seen several instances of apparent change of magnitude or colour among the stars of the same Catalogue, but I will here mention only two in which my observations have been amply verified.

No. 91 is described as very red, and of the 7·3 mag., but on February 1, 1872, it appeared to me as scarcely tinged with red, and from the $7\frac{1}{2}$ to the 8th mag. This observation was confirmed by Mr. Knott, who gauged it 7·5 to $7\frac{3}{4}$, and described it as "not a decided red, but rather golden with ruddy cast." On January 24, 1873, it appeared to me larger, and of a pale but a decided red. I then estimated it at 7 to 7·5 magnitude. In the case of this star the differences in the observed magnitudes may be within the limits of ordinary error of observation; but I feel pretty certain that at least changes in colour have taken place, and this, I should say, supports the probability of change in magnitude.

The second instance of change to which I will now refer, is found in No. 241. This is one of Lamont's stars, and is described as "rubra, 8·5 m."; but on September 24, 1873, it appeared

to me an orange star, not less than from 6 to 6·5 magnitude. Since then I estimated it on different occasions as a little above or below those limits; but the differences are too small to be received as certain without actual measurement. The question of variability depends, therefore, on the accuracy of the original record. My observations have been borne out in the main by Mr. Lynn and other gentlemen of the Royal Observatory, Greenwich, who found the star in last October to be from 6 to 6·5 magnitude.

On the Double Star ν Ceti (Struve 281). By Capt. W. Noble.

Unless Admiral Smyth was more than ordinarily inaccurate in his description of this star, which is the 102nd object in the *Bedford Catalogue*, its minute companion must have increased most notably in magnitude since the date of the observation of it which he there records. (*Celestial Cycle*, Vol. II., p. 64.) Describing the aspect of this asterism at the epoch 1833·88, he says,—speaking of the little star 6″ distant from ν, "this very delicate object is one of those marked by Σ 'difficilis;' and not without reason, for the *comes* can only be seen by glimpses on ardent gazing." It must be borne in mind that this account has reference to the appearance of the object under discussion in the late Admiral's 5·9-inches Tulley achromatic.

On page 211 of the 2nd edition of that most invaluable little book, Webb's *Celestial Objects for Common Telescopes*, I find its author saying with regard to the little companion, "Sm. saw this as a glimpse star. I found it easy with $5\frac{1}{2}$ in 1861."

Now, on the night of Monday last, December 8, while examining the heavens for another purpose, I got ν *Ceti* into the field of view of the telescope, and, to my surprise, saw its companion instantly and steadily with a power of 154. Nay, so comparatively conspicuous was it, that, having viewed it under the higher power, I could see it perfectly well afterwards with one of 74.

My Ross object-glass has, as is pretty well known, an aperture of 4·2 inches only, while that of the Bedford Equatoreal was, as I have previously mentioned, of 5·9 inches in aperture; so that the ratio of the area of my object-glass to that of Admiral Smyth is only $\frac{17·64}{34·81}$, or roughly, one-half; and yet I can see at once and without difficulty, a star which he could only perceive "by glimpses on ardent gazing." It seems then to me an obvious inference that the object in question must be a well-marked variable.

Forest Lodge, Maresfield, Sussex,
1873, *December* 11.

The November Meteors. By the Rev. S. J. Perry.

A constant watch for meteors was kept up during the whole night from November 11, until midnight of November 15. On the first two nights the sky was generally about half covered with cloud, on November 13 seven-tenths were obscured, and it was completely overcast on November 14. The moonlight also interfered a good deal in the early hours, but 91 meteors were observed. Of these 14 had trains, and none were coloured. The times of greatest frequency were from 9h. to 12h. on November 11, from 9h. to 16h. on November 12, and from 9h. to 12h. on November 13, the numbers being respectively 22, 37, and 13. The paths were all noted, and but few were found to belong to the stream of the Leonides.

On November 27, between 8h. and 10h., six meteors were seen, three of which were of the 1st magnitude; but during the remainder of the night only two others were observed, although the amount of cloud was seldom more than three-tenths until 2 A.M. on November 28.

Stonyhurst Observatory,
1873, *December* 11.

Suggestions for a Search for the Small Stars near Uranus, which Sir W. Herschel may have observed as Satellites.

By A. Marth, Esq.

(*Extract from a Letter to Mr. Lassell.*)

It may perhaps be worth while to call the attention of those observers who have the opportunity of using sufficiently powerful telescopes, to the favourable chances they may have during the present apparition of *Uranus* of contributing something towards the decisive settlement of the question respecting the existence of Sir William Herschel's additional satellites of the *Georgium Sidus*.

Though there is no positive evidence available in proof of the real existence of these satellites, and though there can be no doubt that Herschel has not been sufficiently cautious in assuming their existence from the very questionable negative evidence at his disposal, it is only right and due to his memory, that this evidence should be fully sifted and his mistaken inferences properly traced to their true sources. You are aware, that, for years past,

I have been desirous and ready to do this, whenever I should get a fair opportunity; but such an opportunity I have not got. The search for the little stars, which Herschel must have mistaken for satellites, may of course be undertaken at any time and by any observer, who can make use of a powerful telescope in a suitable atmosphere, and who will take the necessary trouble. During the present apparition of *Uranus*, however, observers may take part in the search with no more trouble than that of making careful eyedrafts of the groups of stars in the neighbourhood of the planet itself. For, during the next months, the geocentric place of *Uranus* in the heavens will be only some twenty seconds south of that in which it appeared in 1790 at a (less than) three days' later date, so that the planet in its retrograde course will pass on the nights of January 15 and February 6, 1874, the same stars which it passed on the evenings of January 18 and February 9, 1790, and some of which were then supposed to be additional satellites. It seems certainly desirable that the opportunity for recovering these little stars, and also for ascertaining the effect of the neighbourhood of the planet upon their visibility, should not be allowed to slip away unused; and I therefore suggest to you to call attention to it at the next meeting of the Royal Astronomical Society. It may be well to state expressly, that the telescope employed by Herschel in these observations was his 20-foot reflector, so that there are telescopes enough in the world which ought to be capable of showing what he has seen. With which of them, however, *Oberon* and *Titania* may be really observed, and which of them may be capable of at least showing glimpses of *Ariel* and *Umbriel*, is a question, to which at present no answer can be given, but which might be fairly answered, if observers would be good enough to make simple sketches of the groups of stars surrounding the planet and along its track, at every favourable opportunity during the present apparition, and to send their sketches or copies to the Society. I should be most willing to subject these sketches to the needed examination, and to compute the necessary ephemerides of the known satellites. However, in order to leave observers entirely unbiassed, it will be better not to publish ephemerides before-hand. As regards the two faint satellites, *Ariel* and *Umbriel*, it is not likely that they could be seen with any telescope which does not readily show the satellite of *Neptune*, when it is not too far from its greatest elongation; in fact, they do not seem ever to have been really observed except with your own instruments. But I need not say more on the subject now.

A Third Catalogue of 76 New Double Stars, discovered with a 6-inch Alvan Clark Refractor. By S. W. Burnham, Esq.

(*Communicated by Mr. Dunkin.*)

The accompanying list is the result of a search for new double stars since the preparation of the Catalogue in the *Monthly Notices* for May 1873. The 6-inch Alvan Clark refractor previously described has been used exclusively. In the beginning I limited the distance of doubles to be noted as new to 10″. A few exceptions were made in the case of prominent stars, and where additional companions to well-known doubles were found. In the present observation, I have rarely paid any attention to double stars where the estimated distance exceeded 5″, rejecting also faint pairs of below the 9th magnitude unless connected with a brighter star. With Struve's limit of separation, 32″, a very large number of uncatalogued double stars could be easily found; and if, in addition, faint pairs were noted like those comprising the larger portion of the Catalogues of Sir John Herschel, the number which could be added would be still greater, and only limited by the time necessary to observe their places.

Most of the following are sufficiently close and difficult to be interesting. The distances of fifty-five are within 2″, and of that number about one-fourth will not exceed 1″. The power used almost constantly has been 212; and I believe every double has been discovered, or suspected with that eye-piece, a higher power being used only on very close objects. My next lower power is 125; but it is of no use on really difficult double stars of the class sought for; and my experience has been that when the higher powers cannot be used to advantage, it is in vain to look for anything new.

During the period covered by these observations I noted, altogether, as being possibly not already known, 199 double stars, with an approximate determination of the places. Of this number, 76, forming the present Catalogue, were ascertained to be new. The remaining 123 are distributed among discoverers as follows:—

Struve . . .	75
Otto Struve . .	35
Herschel I. . .	5
Herschel II. . .	2
Alvan Clark . .	3
Dawes . . .	1
Burnham. . . .	2
	123

From this it will be seen that rather more than one-third of the stars entered really proved to be new. Of course, it will be understood that this is only a small portion of the number picked up; but my practice has been to give no attention to stars unless at least moderately difficult under average atmospheric conditions. Occasionally, perhaps, if the place had been determined the double might have been found to be new, but the time saved more than compensates for the probable omissions. In my earlier observations, the proportion of new objects was very much less; but practice has enabled me to know better what to look for, and to decide with some certainty whether or not the star observed is likely to be new; and I think this ratio would now hold substantially good. The value of the various double star catalogues will be seen from a comparison of the number of double stars found and identified in them. *Mensuræ Micrometricæ*, which contains six times as many stars as Otto Struve's Catalogue, furnishes but twice as many, while in all the rest of the works on this subject, embracing between 4,000 and 5,000 stars, there are but eleven identified.

My observations have been largely south of the zenith, and particularly in the vicinity and south of the equator, and this will account for the undue proportion of southern stars. More attention recently, however, has been given to regions further north, with the result of discovering some very interesting pairs. A more careful examination has also been given to previously known doubles encountered from time to time, and in several instances the principal star has been found to be again closely double. Perhaps the most prominent of these, although not the most difficult, is *β Delphini*. As this star has been observed by Struve, both Herschels, Smyth, Dembowski, and others, without the duplicity of the bright star being detected, there is a possibility of its having opened within a few years. Although by no means a severe test for a 6-inch aperture, it is still difficult enough to have been missed, unless very carefully examined. This is not the case with Σ 2793; and it seems impossible for Struve to have overlooked it, unless much closer then than now. 51 *Aquarii* is closer and much more difficult than either. In this part of the heavens generally, there is a great scarcity of close doubles in *Mensuræ Micrometricæ*. Several have been found, given in this and the preceding lists. The close pair of H. 2867 is an exceedingly difficult object, and has no counterpart among Sir John Herschel's double stars. It is a remarkable fact, and hardly accounted for by the sweeping power employed, that the seven catalogues of Sir John Herschel, which, with *Cape Observations*, comprise more than 3,800 double stars visible in this latitude, do not contain a single very close double star, and but six where the estimated distance is down to 1″. How comparatively little there is in them likely to receive any attention in searching for new objects with the limit I have adopted as to distance and magnitudes, will be apparent from an inspection of

the statement above, giving the original observers of the double stars noted from time to time, and found to be already known.

I would call particular attention to *ν Scorpii* (No. 120), which I have very little doubt is an exceedingly close and difficult double star. I examined it several times under the most favourable circumstances, but could not get rid of an apparent elongation of the principal star in a direction nearly north and south. A power of 410 was used, and the utmost care taken in focussing. *β Scorpii* is an admirable object for this purpose, having about the same declination, and also a companion in the same general direction as the more distant companion to *ν Scorpii*. *β* was always seen with a perfectly round and sharply defined disk, and turning the instrument at once on *ν*, it did not seem possible that the slightly wedge-shaped elongation could be an illusion. Not being able to settle the question positively with my aperture, I requested Professor. C. A. Young to examine it with the splendid 9·4-inch Clark refractor of the Dartmouth College Observatory, and he kindly did so. He informs me that he observed it on several occasions, generally under unfavourable conditions; but the last time the air was pretty steady, and while the elongation might be due to atmospheric dispersion, he was rather inclined to think it was double, although he could not even notch it. In the mean time I had tried it again on a number of superb nights, but with no further result than tending to confirm the previous suspicion. From its great southern declination it would necessarily be a very difficult object in this latitude; and it is only when the air is unusually steady that a sufficiently high power can be used to show it. I trust that some observer in the Southern Hemisphere will give this star a careful examination. I shall certainly be much disappointed if it prove to be single. As a wide pair this star is H V. 106 (= Sh. 220 = Σ C.P. 509). Jacob at Madras in 1847 found that the companion was also double, and that is now a very easy object. Should the principal star also prove to be double, it will make by far the most interesting known quadruple in the heavens.

The Nebula given at the end of this Catalogue may possibly not be new, but I have not been able to find it in any of the works I have access to. At the first glance it was taken for a close double companion to Argelander's star. With a large aperture, it will doubtless be found to be a very interesting and curious object.

The distances and angles are in all cases estimated. The numbers are continued from the last Catalogue.

Chicago, U.S., 1873, *November* 12.

No.	Designation.	R.A. 1880. h m s	Decl. 1880. ° ′	Position est. °	Distance est. ″	Magnitudes.	Discovered 1873.	Notes.
107	Arg. (+62°) 93	0 24 28	+62 41	360	2	9, 10	Sept. 8	A faint and not very interesting pair. About 25′ *n* of *κ Cassiopeiæ*. One of four stars in the same field, forming an irregular line.
108	Arg. (+62°) 107	0 27 43	+62 15	10	5	7½, 10	Sept. 8	A third star at 240° 40″. It is 1^m 22^s *f κ Cassiopeiæ*. An 8 m. star in the field *s p*, has a 15″ companion.
109	Ceti 91 A and B C	0 30 26	−17 37	355	100	7,	Oct. 23	A distant double companion.
	B and C	...	...	340	5	10, 10		
110	Ceti 187	1 14 4	−16 32	25	1·8	7, 8	Oct. 28	An elegant but easy pair.
111	Anonyma	10 45 10	− 8 28	360	1·8	9½, 9½	April 25	Small and rather difficult pair, some distance *s f* 41 *Sextantis*. It is 13^s *f* Weisse x. 791, from which its place is determined.
112	P. xii. 243 A and B	12 54 46	+19 1	345	155	6½,	June 3	A pretty double companion to a bright star in *Coma Berenices*. Erroneously called Weisse xii, in Heis' Catalogue.
	B and C	...	...	290	2	8, 9		
113	Arg. (+12°) 2597	13 23 9	+12 6	210	1·6	8½, 12	June 3	I have very little doubt of the duplicity of this star, although it was never well seen, and if double is one of great difficulty. It is 39′ exactly *n* of 71 *Virginis*.
114	Weisse xiii. 438	13 28 0	− 8 0	130	1·8	8, 8	May 30	A fine and not difficult pair, about 1° *s p* 81 *Virginis* (Σ 1763). The *s f* star of a pair in the finder.
115	L. 25365	13 39 24	+10 29	220	1·3	8, 12	June 1	An excessively difficult and unequal pair in *Virgo*; seen well only by the most careful preparation and attention.

No.	Designation.	R.A. 1880. h m s	Decl. 1880. ° ′	Position est. °	Distance est. ″	Magnitudes.	Discovered 1873.	Notes.
116	L. 26177	14 13 3	− 13 9	280	2·5	8, 9	May 15	A pretty little pair 20′ *n* of λ *Virginis*; very easy.
117	L. 26481	14 24 41	−15 5	90	2	8, 9	May 22	Another pair in *Libra* similar to the last.
118	No. 4, Z. 205	14 47 5	−16 0	300	1·7	9, 10½	June 1	A very difficult object, requiring good weather to be well seen. It is 2½ᵐ *f* and 28′ *s* of α *Libræ*. Place from Argelander's Southern Zones.
119	L. 27454	14 59 11	− 6 33	300	1	8, 9	May 4	A fine pair in *Libra* in the vicinity of δ *n f*. It has a minute companion *s f*.
120	ν Scorpii A and B	15 5 1	−19 9	360?	0·3?	4.	June 14	The principal star of this well-known triple strongly suspected to be a very close double. (See further reference in the introduction to this paper.)
121	B.A.C. 5163	15 32 19	−27 16	270	1·5	7, 7	May 30	An elegant pair 2ᵐ 55ˢ *f* and 29′ *n* of 39 *Libræ*, and not difficult under favourable conditions.
122	L. 28495	15 32 58	−19 23	210	2	7, 7½	May 31	This very easy pair precedes κ *Libræ* 2ᵐ 3ˢ, and is 5′ *s*.
123	No. 25, Z. 245	16 47 29	−21 51	210	2	8, 9	June 16	In *Ophiuchus*; place from Washington Meridian Zones.
124	L. 31224	17 3 59	− 0 37	250	1	8, 10	May 31	A very difficult pair in *Ophiuchus n f* a 6 m. star (B.A.C. 5774), but, finally, perfectly seen.
125	P. xvi. 311	17 4 43	−26 53	90	1·5	7, 11	June 13	An exquisite pair 3ᵐ 16ˢ *p* the well-known double, 36 *Ophiuchi*, and 28′ 18″ *s*. The colours of the components are strikingly contrasted when the air is steady enough to give a sharp definition; but it is an object of very great difficulty and requires careful attention. This is B.A.C. 5789. There is an 8 m. star nearly north and almost exactly *p* 36 *Ophiuchi*, which may be a very close equal pair.

No.	Designation.	R.A. 1880. h m s	Decl. 1880. ° ′	Position est. °	Distance est. ″	Magnitudes.	Discovered 1873.	Notes.
126	P. xvii. 43	17 12 53	−17 38	250	1·8	6½, 9	June 15	B.A.C. 5839. Another fine pair in *Ophiuchus*, but comparatively easy. Very nearly visible to the naked eye; rated 6 m. in Lalande, and 5·5 in Washington Catalogue of Stars. A splendid object.
127	L. 31454	17 13 24	−27 13	90	5	8, 10	June 6	The *p* of two small stars 6′ or 7′ apart, and some distance *s f* 36 *Scorpii*.
128	B.A.C. 5879	17 19 33	−26 9	340	4	7½, 10	June 6	This is L. 31668, the declination of which is 4′ 23″ more than that in B.A.C.
129	P. xvii. 100 = B.A.C. 5896	17 21 15	−25 25	100	1·5	7½, 8	June 6	A fine pair *s f* the last; an elegant object.
130	90 Herculis	17 49 22	+40 3	10	1·7	6, 11	June 18	The minuteness of the comparison with its closeness to the principal star makes it tolerably difficult for a 6-in. under ordinary conditions. It is a beautiful pair when well seen.
131	L. 33443	18 3 50	−15 38	270	2	7½, 10	June 16	A pretty pair in *Sagittarius*.
132	B.A.C. 6158	18 4 7	−19 52	60	0·8	7, 7	June 6	An elegant pair about 1° 25′ *n p* μ *Sagittarii*; perfectly seen with disks just in contact.
133	B.A.C. 6261	18 20 15	−26 42	270	1·8	7½, 7½	July 6	The *p* star of a small equilateral triangle of 7 m. stars, about 1° 20′ *s* of λ *Sagittarii*. The other stars of the triangle have distant companions.
134	Arg. (+46°) 2484	18 22 0	+46 50	140	1·3	7½, 10	June 6	A fine and somewhat difficult pair in *Lyra*. The wide pair 1ᵐ 7ˢ *f* is OΣ 352. The fifth new double star found to-night.

No.	Designation.	R.A. 1880. h m s	Decl. 1880. ° ′	Position est. °	Distance est. ″	Magnitudes.	Discovered 1873.	Notes.
135	Scutum Sob. 45	18 31 16	−14 6	210	2	7, 15	June 14	One of the most difficult double stars of the kind I have ever found or seen, from the excessive inequality of the components. It requires the utmost care in focussing, etc., to be seen, and is far more difficult than the companion to α^2 *Capricorni*. It was only after repeated examination on the finest nights that I could be sure of the existence of the small star.
136	Weisse xviii. 893	18 37 0	+ 5 37	10	4	9, 10	Aug. 20	Of no particular interest, 49^s *p* OΣ 361 and 6′ *s*.
137	Weisse xviii. 1503	18 49 47	+37 14	140	1·5	8, 8	June 8	An easy pair, 17^s *p* and 24′ *n* of δ^1 *Lyræ*.
138	L. 36013	19 6 37	−14 39	270	1	7½, 10	Aug. 16	Splendid but excessively difficult pair in *Sagittarius*. A good example of a close and at the same time unequal pair.
139	Aquilæ 59	19 7 11	+16 39	130	0·5	7, 7	Sept. 7	Very close and remarkably difficult pair. It is the *f* star of a wide pair. OΣ 368; a 0″·9 pair in the finder *s f*, is comparatively easy.
140	L. 36185 A and B C	19 10 12	−11 11	330	30	8,	Aug. 22	Star in *Aquila* with a faint double companion.
	B and C	...	...	210	3	14, 15		
141	H. 2867 A and B	19 16 50	+22 17	85	0·4	7½, 8½	Sept. 7	A and C constitute the double star H. 2867, but Sir John Herschel overlooked the duplicity of the principal star. The 15 m. companion is readily seen. The close pair one of the severest tests of the kind in this list. It is L. 36553.
	A and C	...	...	329·5	20 ±	15		
142	Aquilæ 106	19 21 30	−12 23	145	1·4	7, 7+	June 28	Very easy, but still an interesting pair. It is L. 36712.

No.	Designation.	R.A. 1880. h m s	Decl. 1880. ° ′	Position est. °	Distance est. ″	Magnitudes.	Discovered 1873.	Notes.
143	L. 37049	19 26 39	+49 15	200	2	8½, 10	June 8	In the finder with θ *Cygni* s p.
144	Arg. (+30°) 3664	19 33 3	+30 5	185	4	9, 9	June 29	In *Cygnus*, n p φ *Cygni*.
145	L. 37464 A and B	19 36 31	+30 26	280	0·9	7, 10	Aug. 26	A most elegant triple, but the close pair extremely difficult from the great difference in brightness of the stars. This star is about 45′ from φ *Cygni* n f.
	A and C	...	...	175	20	11		
146	L. 37544	19 40 5	−20 10	290	1	9, 11	July 21	In a low power field with *f*(56) *Sagittarii* 43^s *f* and 7′ 26″ s. It is a very difficult object from the smallness of the stars.
147	Arg. (+31°) 3770	19 42 15	+31 48	285	6	9, 11	Oct. 9	An uninteresting double. About 50′ s of H.N. 110, the Decl. of which in Sir John Herschel's Catalogue is 11′ too great. It is identical with S. 726.
148	L. 37779	19 45 27	−10 40	310	1	8, 9	July 21	Another close pair some distance n f 51 *Aquilæ*.
149	L. 38105 A and B C	19 52 46	+16 10	280	125	7,	Sept. 19	A companion to the companion; 18′ s of 11 *Sagittæ*.
	B and C	...	...	200	2·5	11, 15		
150	Σ C.P. 663 = S. 738 A and B	20 5 56	+33 17	112·2	41·86	8, 8·9	Aug. 17	It is singular that Struve and South should have missed the close star, as it is quite plain with 6-in. South's measures of A and B are given. By reason of the distance it is excluded from *Mensuræ Micrometricæ*. The principal star is Weisse xx. 176–7.
	B and C	...	...	190	2	11		

No.	Designation.	R.A. 1880. h m s	Decl. 1880. ° ′	Position est. °	Distance est. ″	Magnitudes.	Discovered 1873.	Notes.
151	β Delphini A and B	20 31 55	+14 11	355	0·7	3½, 5	Aug. 8	It is somewhat remarkable that this splendid pair should have escaped notice. It has been observed by nearly all the well-known double star observers. With an 11 m. companion at 336°·6, distance 34″·64 (Dembowski 1864·9); it constitutes Σ 2704 (= H. iv. 35). Sir William Herschel added a 14 m. companion at 15″, and both were measured by Smyth. Many of the pairs in this list are far more difficult than the close pair.
152	Oephei 55	20 39 18	+56 57	130	0·4	6¾, 8½	Aug. 8	A very close and excessively difficult pair; a short distance *s p* a 5 m. star (P. xx. 322). A pretty severe test. It is Radcliffe 4932.
153	B.A.C. 7187	20 40 10	−26 51	270	1·5	7, 10	Aug. 4	Fine pair in *Capricornus*, and not very difficult. The wide pair 27′ *s* is H. 5220.
154	Taylor 9641	20 46 6	−16 37	55	2·5	8, 10	Aug. 17	L. 40292. In *Capricornus*, 4^m 52^s *p* a 6 m. star (P. xx. 386).
155	Arg. (+50°) 3215	20 48 25	+50 58	40	0·6	7½, 7½	June 13	A splendid double in *Cygnus* 2^m 18^s *f* and 30′ *s* of Σ 2732 (= H. ii. 100 = S. 767). Not very difficult as the stars are very nearly equal. The distance may be underrated.
156	Groombridge 3369	20 57 39	+46 6	260	1·2	8, 11	June 28	This very unequal and difficult pair is 29′ *n* of 60 *Cygni* (OΣ 426). There is a minute distant companion north. Place from Radcliffe Catalogue.
157	Aquarii 43 = Σ 2752 A and C	21 0 29	−14 24	90	20	7, 14	Aug. 18	A minute companion three or four times the distance of Struve's star.

No.	Designation.	R.A. 1880. h m s	Decl. 1880. ° ′	Position est. °	Distance est. ″	Magnitudes.	Discovered 1873.	Notes.
158	L. 40984	21 1 37	+47 19	320	12	8, 16	July 20	The companion only well seen with high powers. It is 37ˢ *p* the supposed new Nebula given at the end of this list.
159	L. 41178	21 6 19	+47 2	330	1·2	8, 10	July 6	The north star of a wide pair 3ᵐ 51ˢ following *f*² (63) *Cygni*. An elegant object.
160	L. 41242 A and B C	21 7 48	+45 13	155	60	8,	July 6	Star with a double companion.
	B and C	...	...	140	2	11, 11		
161	Weisse xxi. 197 A and B	21 10 53	− 5 45	330	90	9,	Aug. 23	Star with double companion; in the finder with 15 *Aquarii*, *n p*.
	B and C	...	...	300	2·5	12, 12		
162	Arg. (+35°) 4461	21 12 14	+35 16	80	1·5	8, 8	June 26	A pretty pair about 50′ *n* of *υ Cygni* (OΣ 433).
163	L. 41386	21 12 47	+11 4	260	1	7, 10	...	A remarkably fine double star in *Equuleus*. The great inequality of the components with their closeness makes it a rather difficult pair. The stars L. 41438 (6½ m.) and L. 41429 (7 m.) do not exist. Probably they are derived from some observation of 41401 and 41386 from which they differ exactly 1ᵐ in R.A.
164	Σ 2793 A and B	21 19 12	+ 8 52	255	0·7	7½, 7½	Aug. 24	The wide pair is Σ 2793, but the principal star is again double. Struve, whose measures of C are given, could hardly have overlooked the close pair if not closer than now. This star is L. 41645.
	A and C	...	...	242·2	26·51	10		
165	L. 41954	21 27 54	− 3 59	180	6	8½, 10	Aug. 3	The *s* and smaller of two stars forming a pair in the finder.

No.	Designation.	R.A. 1880. h m s	Decl. 1880. ° ′	Position est. °	Distance est. ″	Magnitudes.	Discovered 1873.	Notes.
166	Arg. (+59°) 2396	21 30 17	+59 47	260	1·2	7½, 11	Aug. 4	A very fine pair 2m 35s *f* a 5 m. star (B.A.C. 7495). Difficult except in a very steady air.
167	Cygni 363	21 31 1	+29 31	110	1·5	7, 12	July 6	A most difficult pair from the extreme minuteness of the companion and its nearness to the bright star; a very elegant pair under favourable circumstances. Very much like No. 135, but not so difficult. It is P. XXI. 215.
168	Taylor 10162	21 47 7	−20 35	80	4	8, 9	Aug. 18	L. 42642. Another minute companion *f*.
169	O. Arg. S. 21760	21 50 49	−21 43	280	1·8	8½, 8½	Aug. 18	The *s* star of a wide pair, 95″ apart, is a pretty and not very easy double. A 6 m. star (B.A.C. 7649) follows 1m 13s. The 7 m. star 1m 35s *p* is H. 3065, a wide unequal triple. The *p* star of the new double is a little the smallest. Place from the Washington Catalogue of Stars.
170	L. 43158	22 2 31	−19 4	75	2	8½, 8½	Aug. 14	This beautiful little pair is a distant companion to 35 *Aquarii*. P.=40°; D.=160″. There is a minute star between it and 35 *Aquarii*. The wide pair in the field *n f* is H. 3092.
171	L. 43350	22 7 51	−21 38	270	10	8, 15	Sept. 30	This star, which is a distant companion to 41 *Aquarii* (H.N. 56), is 10s *f* and 2′ 44″ *n*, and has a very minute attendant at about twice the distance of that between the components of 41 *Aquarii*. It is spoken of by Webb as a 7 m. star; in Lalande, 9 m.

No.	Designation.	R.A. 1880. h m s	Decl. 1880. ° ′	Position est. °	Distance est. ″	Magnitudes.	Discovered 1873.	Notes.
172	51 Aquarii = H.V. 95	22 17 52	− 5 27	40	0·5	6, 6	Sept. 15	This star with three very distant companions constitutes the double H.V. 95 (= Σ C.P. 748), but both Herschel and Struve missed the close pair. When first found it was thought to be only moderately difficult, but later observations make it a pretty severe test for a 6-inch.
173	Arg. (+ 36°) 2776	22 22 23	+ 56 35	250	2	8½, 12	Aug. 20	A delicate unequal pair in *Cepheus*, 3^m 22^s *p* a wide pair (O Σ 473). There is a 10″ pair of 10 m. stars in the field *p*.
174	L. 43888	22 22 58	− 10 17	280	5	8½, 13	Aug. 28	An unimportant pair in *Aquarius*; the most northern of three stars.
175	Arg. (+ 74°) 970	22 29 49	+ 74 24	120	1·5	9½, 9½	July 24	An extremely difficult pair found in looking for H. 1761, one of Herschel's suspected doubles. It cannot be that pair, as it differs in R.A. about 12^m, and 9′ in Decl., with an entirely different position angle. I could not find any double in H.'s place. The new pair is 4^m 51^s *p* B.A.C. 7907 (6·5 m.) and 21′ *s*.
176	Arg. (+ 38°) 4842	22 35 54	+ 38 57	40	1·8	9, 10	Sept. 2	A pretty little pair 1^m 35^s *p* and 10′ *s* of the fine pair Σ 2942. The three double stars Σ 2942, O Σ 478, and H. 1803 are identical.
177	No. 22, Z. 145	22 45 55	− 22 20	280	2	7½, 8	Sept. 8	In *Aquarius*. Place from *Washington Meridian Transit Zones*.
178	Aquarii 252	22 48 57	− 5 38	310	0·6	6, 8	Sept. 8	A splendid close pair in *Aquarius*, and perfectly seen though not divided. This is a naked-eye star, and is P. xxii. 250 (B.A.C. 7986).

No.	Designation.	R.A. 1880. h m s	Decl. 1880. ° ′	Position est. °	Distance est. ″	Magnitudes.		Discovered 1873.	Notes.
179	Anonyma	22 55 48	−22 54	115	5	8½,	9½	Oct. 19	An unimportant pair of small stars in *Aquarius*.
180	Arg. (+60°) 2482 A and B	23 2 10	+60 11	200	0·5	7½,	7½	July 27	Another very close and difficult pair (in *Cassiopeia*), with a distant companion. There is also a minute pair in the field *s f*. Σ 2977 is about 36′ north, and OΣ 486 is 3^m 44^s preceding the new pair.
	A and C	...	...	100	40		10		
181	Aquarius 286 A and B	23 7 31	−14 3	300	1·5	7,	11	Aug. 27	The close pair an exquisitely beautiful object; 5^m 15^s *p* and 3′ 56″ *n* of the well-known double, 94 *Aquarii* (Σ 2998). The colours of the two nearest are remarkably fine and strikingly contrasted, the primary being reddish. Both companions are difficult in ordinary weather.
	A and C	...	...	230	15		15		
182	Weisse xxiii. 175	23 10 54	−14 27	45	0·8	8,	8	Aug. 27	The *p* star of a small equilateral triangle. It is 1^m 52^s *p* and 20′ *s* of 94 *Aquarii*. A fine close and rather difficult pair.
—	Nebula	21 2 14	+47 22	...	...		...	July 6	A very remarkable and curious double, or elongated planetary (?) nebula. It is close to a 9·3 m. star, Arg. (+47°) 3289. P. = 245°; D. = 30″. This may have been noted before, but it is not in Herschel's General Catalogue, or Lassell's Catalogue of New Nebulæ. It bears magnifying well, and the elongation could not be seen with less than 400. With a large aperture it would probably be a very interesting object. It is 12′·6 *n* of f^2 (63) *Cygni*.

Observations of the Total Eclipse of the Moon on May 12, 1873. By John Tebbutt, Esq.

The sky, with the exception of a few small hazy clouds on the horizon, was clear throughout the eclipse. I do not remember to have seen the Moon's limb more sharply defined, though the periphery of the shadow was unusually irregular and diffused. The following are the local times of the different contacts, as near as I could fix them:—

	h	m	s	
First contact with the shadow .	7	33	59	Local Mean Time.
Beginning of total phase . . .	8	38	49	"
End of total phase . . .	10	8	50	"
Last contact with the shadow .	11	14	14	"

As early as 6h. 45m. the Moon's lower limb was noticed to be less bright than the upper one, but it was not till about 7h. 19m. that the defalcation of light became marked. At no time during the partial eclipse did the limb or the lunar details become undistinguishable in the telescope, and during the whole of the total phase the Moon was plainly visible to the naked eye. About 7h. 54m. the shadow began to assume the usual coppery tint, with the exception of that portion extending inwards about 3′ from the periphery. This portion of the shadow during the partial phase preceding the total immersion, was of a greenish-grey tint. During the total phase the whole disk, excepting that limb furthest from the centre of the shadow, was suffused with a dull red colour, this limb, however, continued of a bright straw colour. At 7h. 51m. the *Milky Way*, with the exception of the more conspicuous portions, such as the nebula about η *Argûs*, was still invisible. The larger Magellanic cloud, however, was faintly perceptible, but the smaller one quite undistinguishable. The *Milky Way* and the smaller cloud were faintly seen by oblique vision at 8h. 3m., but at 9h. 4m. both the Magellanic clouds and *Præsepe* became distinct. Many telescopic stars were occulted during the totality. The following disappearances were observed:—

	h	m	s	
Star 8th Magnitude . .	8	23	51·5	Local Mean Time.
" 8½ " . .	8	35	45·3	"
" 8 " . .	8	37	5·8	"
" 8½ " . .	8	53	59·3	"
" 9 " . .	9	11	54·3	"

The last observation was only approximate, as the star was extremely faint. At 9 h. 17 m. houses constructed of dark material and distant more than 200 yards from the Observatory could not be distinguished from the surrounding landscape. With a view to test the intensity of the darkness, I at 9h. 27m.

attempted to read the title page of the *Nautical Almanac* for 1873 in the moonlight with the unassisted eye, but could not at the ordinary reading distance distinguish the separate letters of the words "Nautical Almanac." I did not succeed in doing so till 10h. 19m., or about 10 minutes after the end of the total phase. The letters of the title page down to "with an appendix" inclusive, could be readily distinguished at 10h. 43m., but this was not the case with letters as small as those contained in the words "containing elements and ephemerides." The larger Magellanic Cloud and the brighter portions of the *Milky Way* were barely visible at 10h. 45m. Seventeen minutes later they had quite vanished from view. Three minutes after the time of last contact the whole of the title page of the *Nautical Almanac* could be made out with the greatest ease. During the partial phase subsequent to the total immersion, the edge of the shadow was of a deep grey tint approaching to slate-blue, and the interior part of a coppery colour. The observations of the contacts and occultations were made with a refractor of 4½-inches aperture, and 70-inches focal length, provided with a power of about 55. The instrument is the work of Messrs. Cooke and Sons, of York.

Observatory, Windsor, New South Wales,
1873. *August* 14.

Observations of Jupiter's Third Satellite. By John Tebbutt, Esq.

While engaged on the 1st of April last, with my 4½-inch equatoreal in observing the transits of *Jupiter's* first and third satellites, I was much struck with the appearance presented by the latter body. With a power of 265 on the equatoreal I could see that the satellite was at 7h. 40½m. just within the disk. With the same power I also observed the first and last contacts of the first satellite at ingress. These took place at 8h. 3½m. and 8h. 5½m. respectively, but the planet's limb was at the time ill-defined. Although the third satellite was distinctly visible as a bright spot when just within the limb, it afterwards disappeared, being undistinguishable from the disk of its primary. I now removed the eye-piece and substituted for it another having a power of 180. On looking at the planet at 8h. 15½m., I was surprised to find not only the first satellite visible as a bright spot just within the disk, but also a little in advance of it a round and very dark spot, which from its position I immediately concluded to be the third satellite. Retaining the power of 180, I watched the phenomenon till the satellite completed its transit. The most interesting circumstance is, that the satellite maintained its blackness throughout the transit, and that it was difficult to

decide which was the more intense the blackness of the satellite or that of the shadow of the first satellite, which was projected at the same time on the disk, and therefore admitted of direct comparison. The third satellite was very near the centre of *Jupiter's* disk at 9h. 16¼m. At 10h. 13¾m., and for a long time afterwards, I noticed that the shadow of the first satellite was projected on one of the dusky belts of the planet, and was surrounded with a ring of faint light. This phenomenon was probably the effect of contrast. Considering the transit of a satellite as a dark spot to be of rare occurrence, I immediately called the attention of our amateurs to the circumstance, with a request that those who were possessed of suitable telescopes should watch the next transit of the third satellite on the 8th of the same month. One observer attempted to carry out the suggestion, but was unsuccessful owing to atmospheric causes. I was, however, more fortunate, for the satellite was again observed by me to transit its primary as a black spot on the 8th of April and 14th of May. Very full notes, with sketches, were taken during the transits. I determined pretty accurately the time when the satellite began to assume the dark appearance. The following observations were made:—

	Local Mean Time. d	h	m	s
First contact at ingress	April 8	11	5	15
Bisection at ingress	"	11	8	15
Last contact at ingress.	"	11	10	45
Satellite hardly distinguishable	"	11	20	13
Satellite quite undistinguishable	"	11	26	13
Beginning of dark phase	"	11	38	7
First contact at ingress	May 14	6	10	20
Last contact at ingress	"	6	16	20
Satellite visible as a very faint luminous spot	"	6	23	20
Satellite quite undistinguishable	"	6	27	20
Beginning of dark phase	"	6	46	40

By "beginning of dark phase" is to be understood the instant when the satellite first became perceptible as a faint dark spot. Its darkness increased gradually in intensity on both occasions, till at length the satellite became as black as the ordinary shadows in transit.

It appears from communications by Messrs. Denning and Roberts to the *Astronomical Register* and the *Monthly Notices* respectively, that the same phenomenon was observed in connexion with the transit of the fourth satellite on the 26th of March last.

Observatory, Windsor, N. S. Wales,
1873, *August* 14.

Nebulæ discovered and observed at the Observatory of Marseilles. By M. Stéphan.

(*Communicated by M. Stéphan through Mr. Hind.*)

Mean R.A. and N.P.D. for 1873·0

No.	R.A. h	m	s	N.P.D. °	′	″	Comparison-Star.
1	2	8	55·19	61	59	25·2	*a*
2	18	23	38·27	67	10	21·1	*b*
3	18	41	18·79	57	51	8·7	*c*
4	19	51	32·23	57	58	58·9	*d*
5	21	9	53·99	91	21	14·8	*e*
6	22	9	50·36	53	21	13·9	*f*
7	22	10	7·11	53	20	12·1	*f*
8	22	45	11·42	53	35	8·6	*g*
9	22	47	22·35	58	32	39·3	*h*
10	22	55	55·98	63	37	55·2	*i*
11	23	16	6·70	78	48	5·5	*k*
12	23	16	26·36	78	42	43·4	*k*
13	23	32	33·54	102	55	37·0	*l*
14	23	38	59·16	63	23	9·5	*m*
15	23	51	34·73	74	12	39·4	*n*

Notes.

1. Exceedingly small and faint; irregular.
2. Very small and faint; round, with a condensation in the centre.
3. Of moderate extent and of irregular form; exceedingly faint and diffused.
4. Very small and exceedingly faint; surrounds three small stars.
5. Very small and exceedingly faint; this nebula has two condensations on the same parallel. The observation refers to the first point of condensation.
6. Exceedingly small and faint; in contact in the N. with a very small star.
7. Very small and faint; vaporous, with a slight condensation in the centre; a small star projecting.
8. Very small and exceedingly faint; round, with a condensation in the centre.
9. Vaporous; very small and exceedingly faint.
10. Exceedingly small and faint, with a condensation in the centre.
11. Small and exceedingly faint; roundish, but of irregular form; diffused, with a slight condensation in the centre.
12. Irregularly round, of moderate extent; faint and diffused, with a slight condensation in the centre.
13. Exceedingly faint and of moderate extent; irregular.
14. Very faint and minute; roundish, with an excentric condensation.
15. Exceedingly small and faint, with a condensation in the centre.

Mean R.A. and N.P.D. of the Comparison-Stars for 1873·0.

	Name of Star.	Mag.	R.A. h m s	N.P.D. ° ′ ″	Authority.
a	B.A.C. 710	6½	2 11 36·01	61 56 41·7	B.A.C.
b	Lalande 34322	7½	18 26 7·80	67 6 13·0	Lalande.
c	W. B. (2) XVIII. 1221	9	18 40 55·62	57 49 50·8	W. B. (2).
d	W. B. (2) XIX. 1790	8·9	19 55 1·50	57 54 28·0	W. B. (2).
e	W. B. (1) XXI. 131	8·9	21 8 8·57	91 21 25·1	W. B. (1).
f	W. B. (2) XXII. 253	9	22 11 17·04	53 24 9·1	W. B. (2).
g	Lalande 44751	9	22 45 56·17	53 36 27·2	Lalande.
h	W. B. (2) XXII. 1135	9	22 49 56·87	58 32 22·8	W. B. (2).
i	Rümker XXII. 10797	...	22 55 3·47	63 41 6·8	Rümker.
k	W. B. (1) XXIII. 397	9	23 20 56·69	78 45 50·3	W. B. (1).
l	W. B. (1) XXIII. 618	9	23 31 6·68	102 54 15·9	W. B. (1).
m	W. B. (2) XXIII. 868	8	23 41 13·62	63 31 33·9	W. B. (2).
n	W. B. (1) XXIII. 1133	9	23 55 54·18	74 16 46·8	W. B. (1).

Elements of Tempel's Comet of July 3 (*Comet* II., 1873). By Mr. W. E. Plummer.

(*Communicated by Mr. Bishop.*)

The following elements are computed from the observations made at Clinton, U.S. and Ann Arbor on July 5, at Marseilles on August 30, and at Twickenham on October 20. All small corrections are taken into account and the Marseilles observation is represented within the errors:—

C−O	″
$dl \cos b$	−4·2
db	0·0

T June 25·377714 G.M.T.

	°	′	″	
π	306	9	43·15	M. eq. 1873·0
☊	120	54	9·06	M. eq. 1873·0
i	12	43	20·20	M. eq. 1873·0
ϕ	32	58	1·20	
log. a		0·4697541		
log. μ		2·8453752		
Period		1850 days.		

The Observatory, Twickenham,
1873, *December* 9.

Supplementary List of Co-ordinates of Stars within or near the Milky Way. By A. Marth, Esq.

(*Communicated by Mr. Lassell.*)

I enclose a "Supplementary List of Co-ordinates of Stars within or near the *Milky Way*," giving the places of a number of leading stars not contained in the lists published in the *Monthly Notices* for November 1872 and June 1873, so as to give to the whole zone one uniform breadth of 40 instead of only 30 degrees, and to include only those portions of the *Milky Way*, which, according to Heis's *Atlas Cœlestis Novus*, extend beyond this zone. I have added at the end the data for tracing in the map the course of some of the arches of the sphere, which may be wanted. For, though not intended to be inserted in a finished drawing of the *Milky Way*, it may for some purposes be useful to have them inserted in the preparatory maps :—

x in.	y in.	Mag.	Star.	B.A.C.
0·20	7·79	5·3	57 Aquilæ	6822
0·51	−0·29	2	α Ophiuchi	5941
1·34	2·70	5·3	...	wanting
·53	1·83	6	...	wanting
·88	0·42	5	93 Herculis	6094
1·99	7·79	3	θ Aquilæ	6934
2·61	1·73	5·7	...	wanting
·70	0·51	5	101 Herculis	6159
·71	0·17	5	96 „	6110
·84	0·07	4·7	95 „	6106
2·84	0·44	4·3	102 „	6157
3·45	3·29	5·3	...	wanting
3·77	−1·04	3·3	μ Herculis	6021
4·28	−0·69	3·7	ξ „	6084
·36	−0·26	4	ο „	6150
·48	−0·73	4·3	ν „	6087
·63	+0·28	5	ι „	6238
·70	−0·41	5	b „	6147
·72	7·75	5	κ Delphini	7141
·96	2·11	6	...	wanting
4·96	0·28	5	A Herculis	6178
5·90	4·21	5·3	...	wanting
5·91	−1·29	4	θ Herculis	6082

x in.	y in.	Mag.	Star.	B.A.C.
6·02	−0·32	4·7	κ Lyræ	6235
·66	4·78	6	20 Vulpeculæ	6944
6·78	−0·41	5	μ Lyræ	6268
7·00	4·08	6	...	wanting
·38	6·10	5·3	30 Vulpeculæ	7188
·41	3·85	6	...	wanting
7·72	8·11	4·3	1 Pegasi	7418
8·12	0·34	var.	R Lyræ	6475
·15	5·05	5·3	48 Cygni	7131
·48	1·32	5·7	...	6656
·57	7·82	4·7	2 Pegasi	7474
·74	−1·11	5	...	6255
8·79	+0·30	5·3	16 Lyræ	6520
9·05	−1·99	2·3	γ Draconis	6091
·38	+7·98	4	κ Pegasi	7571
·42	−0·24	5·3	...	6470
·47	−0·01	5·3	..	wanting
·55	−0·91	5·3	...	6350
9·73	6·52	6	...	7465
10·18	7·44	5	14 Pegasi	7607
·24	0·24	4	κ Cygni	6623
·30	−0·85	5·3	c Draconis	6395
·33	1·75	5·3	e Cygni	6895
·38	5·78	5	70 „	7462
·54	−1·20	5	d Draconis	6348
·86	−1·51	5	b „	6289
10·95	+3·80	5·3	60 Cygni	7306
11·05	−0·19	5·3	54 Draconis	6601
·15	+2·78	5·3	51 Cygni	7182
·18	−0·86	4·7	o Draconis	6463
11·51	3·15	5·7	...	7294
12·21	2·98	6	...	7365
·39	5·44	5·7	...	7681
·57	3·48	6	...	7483
12·72	−0·62	5	π Draconis	6662
13·07	−0·90	3	δ „	6612
·52	−0·05	5	ρ „	6926
·62	−0·70	5·3	σ „	6735
·85	−0·49	3·7	ε „	6836

x in.	y in.	Mag.	Star.	B.A.C.
13·99	1·53	5·3	6 Cephei	7428
14·41	5·65	5·7	...	7999
15·07	−0·38	5·3	73 Draconis	7156
·33	+5·04	6	...	8107
·45	−0·88	4·3	α Cephei	7005
·66	+0·74	5·3	16 „	7686
15·84	5·77	5·7	18 Andromedæ	8231
16·02	2·31	5·3	...	8039
·09	3·38	5·7	4 Cassiopeiæ	8162
·36	2·24	5·3	ο Cephei	8124
16·39	0·48	5·3	...	7881
17·11	3·23	5·7	10 Cassiopeiæ	8373
·23	−0·67	5	34 H. Cephei	7990
·78	8·24	5·3	32 Andromedæ	173
·85	6·03	4·7	ζ Cassiopeiæ	180
17·89	6·72	5	π „	189
18·00	1·16	5·7	23 „	206
·05	−1·74	2	α Ursæ Minoris	360
·05	−1·12	4·3	43 H. Cephei	262
·15	3·91	5·3	...	239
·43	8·42	4	μ Andromedæ	259
·75	1·44	5·3	40 Cassiopeiæ	468
18·95	0·76	5·7	49 „	608
19·04	8·96	2·3	β Andromedæ	334
·19	4·17	5·3	χ Cassiopeiæ	456
19·54	−0·65	5·3	49 H. Cephei	1211
20·16	7·79	5·3	τ Andromedæ	502
20·63	4·47	5·3	ι Persei	721
21·42	8·01	5·3	58 Andromedæ	649
21·67	8·54	3	β Trianguli	656
22·97	7·27	4·7	16 Persei	871
23·34	7·82	5	17 „	877
23·61	7·65	5·3	24 „	915
24·21	0·18	5·3	31 Camelopardali	1849
24·44	7·38	5	23 H. Persei	1017
25·00	0·55	5	ξ Aurigæ	1854
26·63	0·27	5·3	46 ψ „	2044
·70	8·28	4·3	17 Tauri	1147
·87	8·16	4	27 „	1176

x in.	y in.	Mag.	Star.	B.A.C.
26·96	7·10	5·3	41 Tauri	1262
27·71	7·95	4·7	A ,,	1257
27·81	0·07	5·3	55 ψ^4 Aurigæ	2182
28·15	0·36	5·3	50 ψ^2 ,,	2159
28·46	0·02	5	58 ψ^7 ,,	2223
29·03	7·95	4	δ Tauri	1346
·06	7·81	5	68 ,,	1365
·24	8·31	4	γ ,,	1328
·48	7·95	4	θ^1 ,,	1380
·49	7·96	4	θ^2 ,,	1381
·50	7·85	5	...	1391
·62	8·18	5	π Tauri	1370
·83	7·90	5·3	ρ ,,	1409
·83	7·54	5	σ^2 ,,	1437
29·84	7·58		σ^1 ,,	1436
30·15	0·01	5·7	...	2314
·18	6·72	5·3	...	1526
·36	8·02	4·3	c Tauri	1434
·51	7·26	6	o^1 Orionis	1500
·75	7·19	5	o^2 ,,	1525
30·92	5·20	5	119 Tauri	1726
31·06	0·15	4·7	τ Geminorum	2340
·07	−0·68	4·7	ρ ,,	2464
·13	−0·91	1·7	α ,,	2485
·28	7·65	5	7 π^1 Orionis	1516
·36	7·97	4·7	2 π^2 ,,	1491
·67	8·22	3·7	1 π^3 ,,	1486
·80	−0·25	4	ι Geminorum	2442
·81	−0·42	5·3	b^1 ,,	2467
·86	−0·42	5	b^2 ,,	2469
·90	−1·04	5	σ ,,	2540
31·96	+8·32	4·3	3 π^4 Orionis	1495
32·11	−1·07	1·3	β Geminorum	2555
·11	+5·09	5·3	133 Tauri	1834
·18	−0·61	4·3	υ Geminorum	2493
·30	+0·07	5·7	A ,,	2431
·62	8·55	4	8 π^5 Orionis	1514
·84	−0·76	3·7	κ Geminorum	2551
32·87	8·45	4·7	10 π^6 Orionis	1538

x in.	y in.	[illegible]	[illegible]	[illegible]
33·03	6·64	[illegible]	[illegible]	[illegible]
·09	7·70	5	[illegible]	[illegible]
·23	7·25	[illegible]	[illegible]	[illegible]
·42	7·11	5	[illegible]	[illegible]
·59	7·33	5	[illegible]	[illegible]
33·91	7·59	5	[illegible]	[illegible]
34·04	−0·35	[illegible]	[illegible]	[illegible]
·25	7·41	[illegible]	[illegible]	[illegible]
·40	8·76	3	[illegible]	[illegible]
34·99	8·51	4	[illegible]	[illegible]
35·17	8·77	1	[illegible]	[illegible]
·34	8·33	5	[illegible]	[illegible]
·44	7·93	5	[illegible]	[illegible]
35·60	7·62	[illegible]	[illegible]	[illegible]
36·56	−0·96	[illegible]	[illegible]	[illegible]
47·77	−0·23	4	[illegible]	[illegible]

Co-ordinates for tracing the course of the Equator, of the Ecliptic, and of the Parallels of 30° and 60° Decl.

Equator.			Ecliptic.		
x in.	y in.	x in.	x in.	y in.	x in.
68·88	−1	39·12	33·83	−1	63·26
69·57	0	38·43	33·09	0	63·99
70·22	+1	37·78	32·41	+1	64·67
70·83	2	37·17	31·77	2	65·32
71·42	3	36·58	31·15	3	65·94
0·00	4	36·00	30·54	4	66·54
0·58	5	35·42	29·94	5	67·15
1·17	6	34·83	29·32	6	67·77
1·78	7	34·22	28·67	7	68·41
2·43	8	33·57	27·99	8	69·09
3·12	9	32·88	27·26	9	69·83

30° N. Decl.			30° S. Decl.		
x in.	y in.	x in.	x in.	y in.	x in.
4·32	−1	31·68	49·00	−1	59·00
4·77	0	31·23	47·11	0	60·89
5·26	+1	30·74	45·76	+1	62·24
5·80	2	30·20	44·70	2	63·30
6·39	3	29·61	43·81	3	64·19

30° N. Decl.			30° S. Decl.		
x in.	y in.	x in.	x in.	y in.	x in.
7·05	4	28·95	43·05	4	64·95
7·81	5	28·19	42·39	5	65·61
8·70	6	27·30	41·80	6	66·20
9·76	7	26·24	41·26	7	66·74
11·11	8	24·89	40·77	8	67·23
13·00	9	23·00	40·32	9	67·68

60° N. Decl.			60° S. Decl.		
x in.	y in.	x in.	x in.	y in.	x in.
11·31	−1	24·69	54	4·00	54
11·73	0	24·27	53	4·08	55
12·34	+1·00	23·66	52	4·31	56
13·00	1·79	23·00	51	4·71	57
14	2·68	22	50	5·32	58
15	3·29	21	49·00	6·21	59·00
16	3·69	20	48·34	7·00	59·66
17	3·92	19	47·73	8	60·27
18	4·00	18	47·31	8	60·69

Correction: Vol. XXXIII., page 11, x 21·50 read y 1·74 in. instead of 1·44 in.

On a Spectroscopic Observation of a Meteor. By Herr Nicolas de Konkoly.

(*Communicated by Mr. Browning.*)

On the evening of the 13th of October, my assistant told me there was a long star in the N.E. I came on the platform of my observatory and saw the track of this meteor. I did not see the full length of the track, because there was a great cloud, extending to 15°.

The streak of light the meteor left behind was bright enough to enable me to analyse it with a spectroscope—it was nearly 15′ broad. I applied the excellent meteor-spectroscope made by Browning, and saw very finely the bands of sodium and magnesium.

I found the streak of light with the star-spectroscope made by Browning applied to my refractor, and analysed with it the fading light of the track of the meteor; with this telescope, armed with the spectroscope, I saw the bright lines of sodium and magnesium, and two lines in the red and two in the green.

I observed with the instrument several tubes made by Geissler in connection with a Ruhmkorf coil, and I found that the spectrum of the lightning gas in the Geissler tubes was absolutely coincident with these last four bands.

I had eleven minutes to make this observation. After eleven minutes had elapsed, I could not make observations with the instrument, but I could still see the bands of magnesium very finely with the meteor-spectroscope. The last trace of the meteor track I saw at the expiration of 25 minutes in a comet-finder of Steinheil's.

The meteor probably fell to the earth, but I saw a light in the horizon at 50 miles off, and it may have fallen on a house or a combustible object. I have written letters of inquiry in the direction, but I have not yet received any answer. If I should receive an answer containing information, I will go to the place, and make inquiries, with particulars for publication.

O' Gyalla Observatory, Hungary,
1873, *October* 17.

Post-perihelion Places of Coggia's Comet. By J. R. Hind, Esq.

The following positions of this comet for the latter half of December are derived from elements communicated by Professor E. Weiss, to the Vienna Academy, on November 20:—

At Greenwich, Noon.

1873–4	R.A. h	m	s	Decl. °	′	Log. Δ
Dec. 16	13	32	4	−43	7	9·7482
20	13	33	0	45	7	9·7906
24	13	35	0	46	48	9·8274
28	13	37	8	48	14	9·8594
Jan. 1	13	40	9	−49	28	9·8873

It will be remarked that, as regards position, there would be no difficulty in observing the comet in the Southern Hemisphere, but its faintness may be such, that the only hope of procuring places will be from telegraphing its position and direction of motion on some date during the absence of moonlight to Mr. Ellery, who might then, with the Melbourne reflector, furnish the means for deciding as to identity or otherwise with the comet of February 1818. The observations in this hemisphere extend over too short an interval to be adequate to this purpose. If the period be really as conjectured, about 55 years, the comet must make a close approach to the orbit of *Venus* near the descending node.

Parabolic Elements of the Comet, 1743-4. By Mr. W. E. Plummer.

(*Communicated by Mr. Bishop.*)

The following calculations have all been based upon the reduction of the original observations by Mr. Hind, published in the *Astronomische Nachrichten.* I have likewise adopted the positions of the Sun which Mr. Hind has calculated from Carlini's latest Solar Tables. The difference between Carlini and Le Verrier for that time is very slight.

By using Maraldi's observation of 1743, December 21; Bradley's of 1744, January 24; and the mean of Bradley's and Bliss's observations on February 28, a preliminary orbit was obtained, which served for the corrections of parallax and aberration, and supplied the means of obtaining normal places. It is a curious fact connected with this calculation, that the first approximation to the proportion of the curtate distances of the comet at the first and third observations gave a negative result.

ELEMENTS I.

T 1744, March 1·34370 Paris M.T.

	°	′	″	
π	197	11	43·8	M. eq. 1744·0.
☊	45	45	18·3	
i	47	2	55·9	
log. q	9·3467620			

Motion direct.

The error of the ephemeris computed from these elements on January 24 was

	C − O		C − O
	′ ″		′ ″
R.A.	+ 1 25·9	Decl.	− 1 1·3,

with which ephemeris all the observations collected by Mr. Hind were compared. It is unnecessary to give the individual comparisons. The errors were grouped into six combinations, which showed that the ephemeris required the following corrections:—

Paris M.T.	R.A. ′ ″	Decl. ′ ″
1743, Dec. 27·5	− 0 48·0	+ 0 20·4
1744, Jan. 8·5	− 1 42·3	+ 0 44·7
Jan. 21·5	− 2 11·2	+ 0 51·7
Jan. 31·5	− 1 36·2	+ 1 17·1
Feb. 15·0	− 1 43·8	+ 0 29·3
Feb. 29·0	− 0 19·1	+ 0 20·0

For these times 12 equations of condition were formed, which it is hardly necessary to give here; and the application of the method of least squares showed that the necessary corrections to Elements I. were

$$\begin{array}{lll} dT & + & 0{\cdot}0025815 \\ dq & - & 0{\cdot}0000007 \\ & & \;\;{}' \;\;\;\; {}'' \\ d\pi & + & 0 \;\; 4{\cdot}3 \\ d\Omega & - & 0 \;\; 25{\cdot}4 \\ di & + & 4 \;\; 22{\cdot}7 \end{array}$$

and therefore the improved elements are as follow:—

ELEMENTS II.

T	March 1·3462815 Paris M.T.				
	°	′	″		
π	197	11	48·1	}	M. eq. 1744·0
☊	45	44	52·9	}	M. eq. 1744·0
i	47	7	18·6	}	M. eq. 1744·0
log. q	9·3467607				

Motion direct.

A comparison with the six normal places exhibits the following errors:—

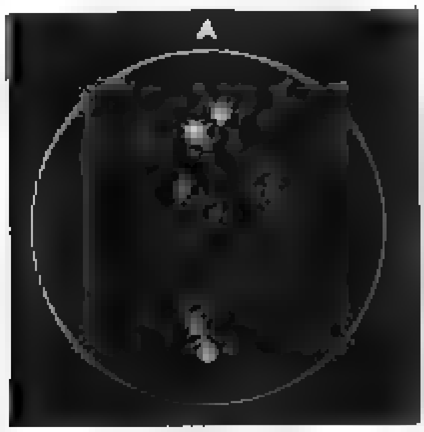
	$d\lambda \cos\beta$	$d\beta$
	″	″
Dec. 27	+19·9	+36·2
Jan. 8	− 5·5	+10·1
Jan. 21	−20·3	−25·0
Jan. 31	+21·6	−20·9
Feb. 15	−34·7	− 6·4
Feb. 29	+21·2	+ 0·1

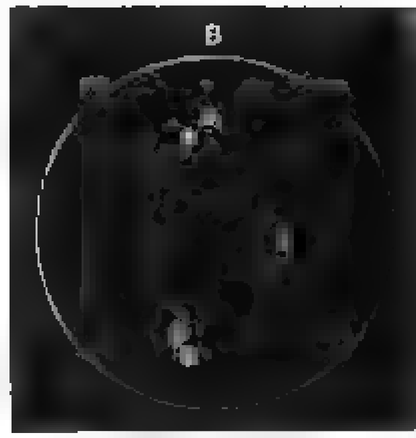
The Observatory, Twickenham,
1873, *December* 10.

On the Position and Magnitude of the Stars between ε and 5 Lyræ. By C. L. Prince, Esq.

Considerable increase of brilliancy is observable in the largest of the three stars which lie between the two pairs of this asterism. There has been, also, great alteration in the relative position of these stars with respect to the principal components. Admiral Smyth, in his remarks respecting this group, says (*Celestial Cycle*, vol. II., page 428), "Between these pairs and *two thirds* over

from ϵ towards 5, are three small stars forming a curve to the south." The following diagram (A) is the one he has given for 1842 epoch, and (B) is one which I made a few evenings since, from which it appears that the central acolyte is more nearly midway between the two pairs than formerly, while the largest forms, with them, very nearly, the apex of a triangle.





The Observatory, Crowborough Beacon,
1873, *December* 9.

If the relative positions and magnitudes of the stars in this interesting group are really changed from those depicted in the *Celestial Cycle*, as pointed out by Mr. Prince, it will be highly important that the fact be confirmed by a set of careful measures of the angular distances and positions of the three intermediate stars with reference to ϵ and 5 *Lyræ*.—[Editor.]

Note by the Astronomer Royal.

In my paper "On the rejection, in the Lunar Theory, of the term of longitude depending for argument on eight times the mean longitude of *Venus* minus thirteen times the mean longitude of the Earth, &c.," inserted in the last number of the *Monthly Notices*, I omitted to allude to Professor Newcomb's valuable paper, "Considerations on the Apparent Inequalities of Long Periods, &c," which contains remarks on the same subject. The observations, however, to which references are made, and the general modes of treatment, are essentially different, and the two papers may be regarded as absolutely independent.

1873, *December* 8.

Professor Newcomb's Tables of Uranus.

It was announced at the last Meeting of the Society that Professor Newcomb's important investigation on the orbit of *Uranus* has been published. It contains a full explanation of the theory of the motion of that planet, together with a well-arranged series of Tables for the calculation of the tabular places. Although a copy of this valuable work has not yet reached the Society, yet we have seen one specially sent to the Astronomer Royal by the author. Before, however, the publication of the Tables, they have been adopted in the calculation of the tabular places of *Uranus* contained in the American *Nautical Almanac* for 1876; and through the kindness of Professor Newcomb, Mr. Hind has been enabled to give the places of *Uranus* in the *Nautical Almanac* for 1877, also derived from the new Tables. From the known care taken by Professor Newcomb in the preliminary investigations, in which he has used the meridional observations of *Uranus* made at Greenwich and Washington down to the opposition of 1871-2, we may confidently anticipate that the new Tables will represent the orbital motion of the planet with a great degree of accuracy.

We may remark here that Professor Newcomb's Tables of *Neptune*, during the few years they have been adopted in the *Nautical Almanac* calculations, have almost perfectly represented the motion of that planet. The observed errors of the Tabular R.A. and N.P.D. are at the present time very small indeed, being no greater than the separate accidental errors found in the observed positions of the fundamental stars.

ERRATA.

Vol. XXXIII. page 169, line 19, *for* 22° 10′ 27″·29 *read* 22h. 10m. 27s·29.

" " 347, " 34, *for* α Geminorum *read* ε Geminorum.

" " 490, " " 491, " " 492, — In heading, 'Year of Observation,' *for* 1871 *read* 1873.

Vol. XXXIV. " 1, line 14 from bottom, *for* page 583 *read* page 533.

" " 31, " 9, *for* 5° *read* 5″.

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXXIV. *January 9, 1874.* No. 3.

PROFESSOR CAYLEY, F.R.S., President, in the Chair.

Charles Becker, Esq., 112 St. Martin's Lane;
James Irwin Coates, Esq., Headingley, Leeds;
Ralph Copeland, Ph.D., Parsonstown Observatory, Ireland;
Wm. Samuel Davis, LL.D., 1 Cambridge Villas, Derby;
Rev. Cosmo Reed Gordon, D.D., 3 Norland Place, Notting Hill;
Major W. Palmer, R.E., Southborough, Tunbridge Wells;
Dr. J. C. Robertson, Monaghan, Ireland;
Capt. F. C. B. Robinson, R.N., Junior United Service Club; and
Rev. Thos. Thistlethwaite Smith, Thruxton Rectory, Hereford,

were balloted for and duly elected Fellows of the Society.

Dr. Edouard Heis, Münster;
Dr. Theodor Oppolzer, Vienna;
Dr. Johann Freidrich Julius Schmidt, Athens Observatory;
and Mons. C. Wolf, Paris Observatory,

were also balloted for and duly elected Associates of the Society.

On a proposed New Method of Treating the Lunar Theory. By Sir George Biddell Airy, K.C.B., Astronomer Royal.

It appears desirable to introduce the explanation of the method now proposed by a rapid survey of the methods hitherto employed.

In the whole range of physical mathematics, there is perhaps nothing more remarkable than the beauty of the geometrical

integrations, in the III. Book of *Newton's Principia*, for the Lunar Inequalities in Latitude and for the Lunar Variation; and the general accuracy of the results. It is clear also, from a few remarks in the 11th Section of the I. Book, and from an unexplained remark on the comparison of the inequalities of the satellites of other planets with those of our Moon in the III. Book, that Newton perfectly understood the origin of what are now called the terms of the second order, by which the Velocity of Progression of the Apse, and the Evection, are so much increased. But Newton published no numerical calculation of those quantities; and the theory was, so far, left imperfect. A more powerful calculus was necessary.

The want was supplied by the Differential Calculus, in the shape in which it was established among Continental mathematicians; and the particular form in which it was applied by Clairaut to the Lunar Theory exhibited at once the power of the Calculus and the ease of applying it. The simple form of Clairaut's differential equations for parallax and latitude opened out the entire process of extending the theory to any degree of accuracy, and showed at the same time the steps by which periodical inequalities of one form are deduced from the combination of periodical inequalities of other forms. I think that scarcely sufficient honour has been given to Clairaut for the formation of this special equation, without which the progress of the theory would probably have been very slow. Even now it is the best form in which a beginner can enter upon the studies of the Lunar Theory. Clairaut's theory gives the time in terms of the arc of longitude described, which is not without advantage in the treatment of equations of long period; but it requires a final reversion of series, in order to give the longitude in terms of the time. Mathematicians in the later part of the present century have preferred a form in which the Moon's ordinates are expressed immediately in terms of the time. I give my adhesion to this method; but at the same time I am anxious to offer my testimony to the value of the process so successfully introduced at a most critical point in the progress of the science.

The next important extensions of the theory were those of Laplace and Damoiseau: both founded on Clairaut's equation; both exhibiting the subordinate equations derived from the comparison of coefficients which are expressed by unexpanded algebraical fractions whose denominators are very complicated (the piles of these fractions, especially in Damoiseau's work, are appalling); both giving the first results in numerical values for the coefficients of numerous arguments which are multiples of longitude; both leaving in great obscurity the process by which the numerical solutions of these algebraical comparisons were obtained; and both giving the final results in terms depending on the time. Damoiseau, however, added to this investigation a work which demands our gratitude: a system of Lunar Tables expressly founded on the aggregation of simple periodical terms

having for arguments different multiples of the time. It was by use of these Tables (with small additions derived principally from Plana) that I conducted the great Reduction of Lunar Observations from 1750 to 1853, and deduced from them the corrections of the principal coefficients. Damoiseau's angular values were all expressed in the centesimal division of the quadrant: a method which possesses so many advantages that I hope for its adoption in future tables.

Plana's work, which followed, was not entirely pure in its method. It commences, for instance, with an application of theorems for the "variation of constants," here introduced with great advantage. But in the more advanced parts it may be described as established on the use of the time as the independent variable, and as exhibiting every coefficient in a series of algebraical terms without denominators. Viewed as leading to an algebraical result, this work was a great advance beyond all which had preceded it; and in numerical accuracy it is probable that something was gained.

I do not advert to the extensive investigations of Lubbock, because they were principally in the nature of verifications, adopting generally M. Plana's system. Nor do I consider the important questions raised by Professor Adams, because they are, in fact, a re-examination of specific points in a received theory. Professor Hansen's theory and tables require mention, principally in explanation of my reasons for almost omitting them from a view of the progress of the science. I attach the highest value to Professor Hansen's discovery of two inequalities in longitude produced by *Venus*; of which one is universally accepted, and the other, though controverted, still appears plausible. And I value the new equation which he introduced in the Moon's latitude. I believe also that the object which Professor Hansen originally proposed to himself, namely, the more rapid convergence of terms, has been (in some measure at least) attained. Yet I think that the general form of his theory, differing so much from the two systems which had preceded it, and presenting little facility for correcting elements from observations, is so far objectionable that it is not likely to be adopted by future lunar theorists; and that its introduction was, in fact, a retrograde step. But, in common with all who are practically concerned with lunar observations, I am grateful for his Lunar Tables, which, embodying the results of his own theory and the Greenwich corrections of elements, and published at a time when the existing tables were running wild, have been most beneficial to practical science.

But there remains one glorious work, almost superhuman in its labour, and perfect beyond others in the detailed exhibition of its results; the Lunar Theory of Delaunay. In this the time is adopted as the independent variable. The masses of undeveloped fractions here exhibited are greater than those of Damoiseau; the development in terms without denominators is more extensive

than that of Plana; and the numerical evaluation of every term is more complete than that of any preceding writer. Some terms to which we should have attached great interest are lost (at least for the present) by the untimely death of M. Delaunay.

Now, in all these works, so far as I have remarked, the following characteristics hold:—

(1). Each investigator has begun his work *de novo*, without making any use of the results of preceding investigators, even with the application of contingent corrections.

(2). Each investigator has used the fractions, in symbolical terms, to which I have alluded; and, by adherence to the symbolical form, has been compelled to expand them in series with rapidly increasing coefficients.

(3). The nature of the steps has compelled the investigators to decide the succession of their terms, not by numerical magnitude, but by algebraical order. And this has produced great inequality of convergence. Delaunay's smaller coefficients are probably correct, as he has exhibited them, to $0''\cdot0001$; but his larger terms converge so slowly that he has been compelled to supplement them by an assumed law of decrease; and they may perhaps be in error by almost $1''\cdot0000$.

(4). The mental labour in these operations is fearfully great. M. Plana once remarked to me, "Quelquefois, Monsieur, ces calculs me font presque perdre la tête."

(5). This labour cannot be alleviated, even in the examination of work done, by an amanuensis or assistant.

In consideration of these circumstances (which I have known, as well from examination of the works of others, as from my private investigations), I have long held the opinion that a Lunar Theory, in which every coefficient is expressed, from the very beginning of the process and throughout, by simple numbers, is very desirable. My ideas on this subject have by degrees assumed an orderly form; and I am now able to exhibit their leading points, as follow:—

(1*). I propose to assume Delaunay's final numerical expressions, for longitude, latitude, and parallax, with the addition of secular equations, as my fundamental numbers. These will be converted into other numerical expressions referred to more convenient units. To every number, as far as I think necessary, will be attached a symbolic term for contingent correction; in some cases considered as varying with the time. In all cases I assume that this correction will be so small that its first power will be sufficient. The secular terms will probably introduce cosines with sines of the same argument.

(2*). I propose to substitute these numbers with symbolical corrections in the equations in which the time is adopted as independent variable. The fractions to

which I have alluded will still occur, but not in a troublesome symbolical form. The greatest complication of denominators will be that of "a number with small symbolical correction attached to it;" which will be instantly converted into two terms without denominator. There will never be an infinite series.

(3*). The order of terms will be numerical; and, as far as I perceive, they will be equally accurate throughout.

(4*). The details of work will be very easy.

(5*). A great part of the work can be intrusted to a mere computer; and probably the whole can be examined, or can be repeated in duplicate, by such assistant.

To these I add,

(6*). I have strong confidence that equations of very long period may thus be examined with great severity, especially when there is reason to suspect that the form of the principal arguments may be slightly changed.

(7*). The result of the comparison of the terms in the mechanical or gravitational equations will be, a great number of equations for determining the numerical values of a great number of small quantities. I anticipate no difficulty in the solution; it is usually sufficient, for the determination of any one of the small quantities, to change (where necessary) the sign of its coefficient, so as to have all its coefficients with the same sign (the sign of the constant term being also changed), and to add all; neglecting all the other unknown quantities. In some cases, however, it may be necessary to treat two of these corrections in combination.

Though very late, I have actually begun a Lunar Theory in the shape which I have described. It is sufficiently possible that I may not be able to complete it; but I desire to leave it in such a state that a successor may be able to take it up successfully. For this purpose, I will enter into some further details as to the steps which I have made.

I. I refer all ordinates to an invariable plane and an invariable line in that plane; for instance, the plane of the ecliptic and the equinoxial line for the beginning of the year 1900.

II. I represent the masses of the Sun, the Earth, and the Moon, by the Greek letters σ, ϵ, μ. It is supposed that they are estimated, as is usual, by the acceleration which their attraction at distance 1 would produce in time 1. The letter σ' is accented to show that it is necessary to apply a term of contingent correction to the assumed Sun's mass, and similarly for ϵ' and μ'.

III. It is easily demonstrated that, to a very high degree of accuracy, the motion of the centre of gravity of Earth and Moon is subject to the same laws as the motion of a planet in that place, and that their action on Sun and planets is the same as if their mass were collected in that place.

IV. Explaining at present only a small part of the adopted

notation: A′, R′, V′, are the true major axis of orbit, true radius vector, and true longitude, of the 'centre of gravity of Earth and Moon,' measured from the Sun; a', r', v', those of the Moon as measured from the Earth; $v_{,,}$ the tropical longitude of the Moon; l′, the latitude of the Moon. The perturbing forces are P, measured from the Earth in the projection of the Moon's radius vector on the fixed plane; T, at right angles to P in that plane, accelerating the tangential motion; and Z, measured from that plane and at right angles to it.

V. The three gravitational equations of motion are the following: in which the left-hand-side contains nothing but ordinates and forces produced by the Earth and Moon considered as points, and the right-hand-side contains nothing but forces produced by the Earth's oblateness and the Sun's disturbance (applicable in the same shape to a planet's disturbance):—

$$\frac{a'^3}{\epsilon'+\mu'}\cdot\left(\frac{a}{a'}\right)^2\cdot\frac{d}{dt}\left\{\left(\frac{a'}{a}\cdot\frac{r'}{a'}\cos \mathrm{l}'\right)^2\cdot\frac{dv'}{dt}\right\} = \frac{a'^2}{\epsilon'+\mu'}\left(\frac{r'}{a'}\cos \mathrm{l}\right)\cdot \mathrm{T}.$$

$$\frac{a'^3}{\epsilon'+\mu'}\cdot\frac{d}{dt}\left\{\left(\frac{d}{dt}\left(\frac{a'}{a}\cdot\frac{r'}{a'}\cos \mathrm{l}'\right)\right)^2+\left(\frac{a'}{a}\cdot\frac{r'}{a'}\cos \mathrm{l}'\cdot\frac{dv'}{dt}\right)^2\right\}$$

$$+\,2\,\frac{a'}{a}\cdot\left(\frac{a'}{r'}\right)^2\cos \mathrm{l}'\cdot\frac{d}{dt}\left(\frac{a'}{a}\cdot\frac{r'}{a'}\cos \mathrm{l}'\right)$$

$$=2\,\frac{a'^3}{\epsilon'+\mu'}\,\frac{\mathrm{P}}{a}\cdot\frac{d}{dt}\left(\frac{a'}{a}\cdot\frac{r'}{a'}\cos \mathrm{l}'\right)+\,2\,\frac{a'^3}{\epsilon'+\mu'}\cdot\frac{\mathrm{T}}{a}\cdot\frac{a'}{a}\cdot\frac{r'}{a'}\cos \mathrm{l}'\frac{dv'}{dt}.$$

$$\frac{a'^3}{\epsilon'+\mu'}\cdot\frac{d^2}{dt^2}\left(\frac{a'}{a}\cdot\frac{r'}{a'}\cdot\sin \mathrm{l}\right)+\frac{a'}{a}\left(\frac{a'}{r'}\right)^2\sin \mathrm{l}=\frac{a'^3}{\epsilon'+\mu'}\cdot\frac{\mathrm{Z}}{a}.$$

VI. For developing the left-hand-sides by substitution of Delaunay's values (subsequently to be furnished with additional symbols of contingent corrections), Delaunay's coefficients are to be converted into simple numbers: those for the powers of $\frac{r}{a}$ or $\frac{a}{r}$ into fractions of unity in which the last retained unit is the ten-millionth part of unity or 10^{-7}; and those for the terms of longitude and latitude into fractions of radius in which the last-retained unit is the ten-millionth part of radius (corresponding very nearly to 0″·02). The decimal cyphers preceding the efficient figures are to be omitted.

VII. For developing the right-hand-sides, it will be necessary to adopt for the last-retained unit the thousand-millionth part of unity or radius, in order to secure the terms which rise greatly by integration. But this amounts practically to the same extent of development as that already mentioned, because two decimal places are supplied by the smallness of the external factor representing the Sun's disturbing force (the value of its principal term being about $\frac{1}{180}$).

VIII. The following appears to be the expression for the right-hand-side of the first equation; where e is the ellipticity of the Earth's surface; $\frac{eq.\ c.\ f.}{grav.}$ is the proportion of centrifugal force at the equator to gravity; c is the Earth's polar semi-axis, and ω is the inclination of the equator to the ecliptic:—

$$+\left[\left(e-\frac{eq.\ c.\ f.}{2\ grav.}\right)\cdot\left(\frac{c}{a'}\right)^2\right]\left(\frac{a'}{r'}\right)^3\cdot\left\{-2\sin^2\omega\cdot\cos^2 l\cdot\sin v_{\prime}\cdot\cos v_{\prime}\right.$$
$$\left.-2\cos\omega\cdot\sin\omega\ \cos l\cdot\sin l\cdot\cos v_{\prime}\right\}$$

$$+\left[\frac{\sigma'\cdot a'^3}{(\epsilon'+\mu')A'^3}\cdot\frac{\epsilon+\mu}{\epsilon+\mu}\right]\left(\frac{A'}{R'}\right)^3\cdot\left(\frac{r'}{a'}\right)^2\cdot\left\{-3\cos^2 l\cdot\cos\overline{v'-V'}\cdot\sin\overline{v'-V'}\right\}$$

$$+\left[\frac{\sigma'\cdot a'^3}{(\epsilon'+\mu')A'^3}\cdot\frac{\epsilon^2-\mu^2}{(\epsilon+\mu)^2}\cdot\frac{a'}{A'}\right]\cdot\left(\frac{A'}{R'}\right)^4\cdot\left(\frac{r'}{a'}\right)^3\cdot\left\{+\frac{15}{2}\cos^3 l\cdot\cos^2\overline{v'-V'}\cdot\sin\overline{v'-V'}\right.$$
$$\left.-\frac{3}{2}\cos l\cdot\sin\overline{v'-V'}\right\}$$

$$+\left[\frac{\sigma'\cdot a'^3}{(\epsilon'+\mu')A'^3}\cdot\frac{\epsilon^3+\mu^3}{(\epsilon+\mu)^3}\cdot\left(\frac{a'}{A'}\right)^2\right]\left(\frac{A'}{R'}\right)^5\cdot\left(\frac{r'}{a'}\right)^4\left\{-\frac{35}{2}\cos^4 l\cdot\cos^3\overline{v'-V'}\cdot\sin\overline{v'-V'}\right.$$
$$\left.+\frac{15}{2}\cdot\cos^2 l\cdot\cos\overline{v'-V'}\cdot\sin\overline{v'-V'}\right\}$$

$$+\left[\frac{\sigma'\cdot a'^3}{(\epsilon'+\mu')A'^3}\cdot\frac{\epsilon+\mu}{\epsilon+\mu}\right]\left(\frac{A'}{R'}\right)^3\cdot\left(\frac{r'}{a'}\right)^2\cdot bt\left\{-3\cos l\cdot\sin l\cdot\sin\overline{v'-V'}\cdot\sin\overline{V'-K}\right\}$$

$$+\left[\frac{\sigma'\cdot a'^3}{(\epsilon'+\mu')A'^3}\cdot\frac{\epsilon^2-\mu^2}{(\epsilon+\mu^2}\cdot\frac{a'}{A'}\right]\cdot\left(\frac{A'}{R'}\right)^4\cdot\left(\frac{r'}{a'}\right)^3\cdot bt$$
$$\times\left\{+15\cos^2 l\cdot\sin l\cdot\cos\overline{v'-V'}\cdot\sin\overline{v'-V'}\cdot\sin\overline{V'-K}\right\}$$

The first line contains the effect of the Earth's oblateness; the second line contains the solar perturbations which first present themselves; the third line contains the principal parallactic terms; the fourth line, succeeding terms of smaller magnitude; the fifth and sixth lines, terms depending on the secular change in the position of the ecliptic, whose effect on the heliocentric latitude of the 'centre of gravity of Earth and Moon' is supposed to be represented by the term $bt \sin \overline{V'-K}$

A general periodical term may be added, to represent perturbation produced by any unrecognised cause.

IX. The right-hand-side of the second equation appears to consist of the following terms:—

$$+2\left[\left(\mathrm{e}-\frac{eq.\,c.\,f.}{2\,grav.}\right)\left(\frac{c}{a}\right)^2\right]\left(\frac{a'}{r'}\right)^4\cdot\frac{d}{dt}\left(\frac{a'}{a}\cdot\frac{r'}{a'}\cos\mathrm{l}'\right)$$

$$\times\left\{\begin{array}{l}5\,(\sin\omega\cdot\cos\mathrm{l}\cdot\sin v_{,}+\cos\omega\cdot\sin\mathrm{l})^2\cdot\cos\mathrm{l}\\-2\,(\sin\omega\cdot\cos\mathrm{l}\cdot\sin v_{,}+\cos\omega\cdot\sin\mathrm{l})\cdot\sin\omega\cdot\sin v_{,}\\-\cos\mathrm{l}\end{array}\right\}$$

$$+2\left[\frac{\sigma'.a'^3}{(\epsilon'+\mu').\mathrm{A}'^3}\cdot\frac{\epsilon+\mu}{\epsilon+\mu}\right]\cdot\left(\frac{\mathrm{A}'}{\mathrm{R}'}\right)^3\frac{r'}{a'}\cdot\frac{d}{dt}\left(\frac{a'}{a}\,\frac{r'}{a'}\cos\mathrm{l}'\right)$$

$$\times\left\{+3\cos\mathrm{l}'\cdot\cos^2\overline{v'-\mathrm{V}'}\;-\;\cos\mathrm{l}'\right\}$$

$$+2\left[\frac{\sigma'.a'^3}{(\epsilon'+\mu').\mathrm{A}'^3}\cdot\frac{\epsilon^2-\mu^2}{(\epsilon+\mu)^2}\cdot\frac{a'}{\mathrm{A}'}\right]\left(\frac{\mathrm{A}'}{\mathrm{R}'}\right)^4\cdot\left(\frac{r'}{a'}\right)^2\cdot\frac{d}{dt}\left(\frac{a'}{a}\,\frac{r'}{a'}\cos\mathrm{l}'\right)$$

$$\times\left\{-\frac{15}{2}\cos^2\mathrm{l}'\cdot\cos^3\overline{v'-\mathrm{V}'}+\frac{3}{2}\cos\overline{v'-\mathrm{V}'}+3\cos^2\mathrm{l}'\cdot\cos\overline{v'-\mathrm{V}'}\right\}$$

$$+2\left[\frac{\sigma'.a'^3}{(\epsilon'+\mu').\mathrm{A}'^3}\cdot\frac{\epsilon^3+\mu^3}{(\epsilon+\mu)^3}\cdot\left(\frac{a'}{\mathrm{A}'}\right)^2\right]\cdot\left(\frac{\mathrm{A}'}{\mathrm{R}'}\right)^5\cdot\left(\frac{r'}{a'}\right)^3\cdot\frac{d}{dt}\left(\frac{a'}{a}\cdot\frac{r'}{a'}\cos\mathrm{l}'\right)$$

$$\times\left\{+\frac{35}{2}\cos^3\mathrm{l}\cdot\cos^4\overline{v'-\mathrm{V}'}-\frac{15}{2}\cos\mathrm{l}\cdot\cos^2\overline{v'-\mathrm{V}'}\right.$$

$$\left.-\frac{15}{2}\cos^3\mathrm{l}\cdot\cos^2\overline{v'-\mathrm{V}'}+\frac{3}{2}\cos\mathrm{l}\right\}$$

$$+2\left[\frac{\sigma'.a'^3}{(\epsilon'+\mu')\mathrm{A}'^3}\cdot\frac{\epsilon+\mu}{\epsilon+\mu}\right]\cdot\left(\frac{\mathrm{A}'}{\mathrm{R}'}\right)^3\cdot\frac{r'}{a'}\cdot\frac{d}{dt}\left(\frac{a'}{a}\,\frac{r'}{a'}\cos\mathrm{l}'\right)$$

$$\times bt\cdot\left\{+3\sin\mathrm{l}'\cdot\cos\overline{\mathrm{V}'-\mathrm{K}}\,\sin\overline{\mathrm{V}'-\mathrm{K}}\right\}$$

$$+2\left[\frac{\sigma'.a'^3}{(\epsilon'+\mu')\mathrm{A}'^3}\cdot\frac{\epsilon^2-\mu^2}{(\epsilon+\mu)^2}\cdot\frac{a'}{\mathrm{A}'}\right]\cdot\left(\frac{\mathrm{A}'}{\mathrm{R}'}\right)^4\cdot\left(\frac{r'}{a'}\right)^2\cdot\frac{d}{dt}\left(\frac{a'}{a}\,\frac{r'}{a'}\cos\mathrm{l}'\right)$$

$$\times bt\left\{-15\cdot\cos\mathrm{l}'.\sin\mathrm{l}'.\cos^2\overline{v'-\mathrm{V}'}.\sin\overline{\mathrm{V}'-\mathrm{K}}+3\cos\mathrm{l}'.\sin\mathrm{l}'.\sin\overline{\mathrm{V}'-\mathrm{K}}\right\}$$

$$+2\frac{dv'}{dt}\times\left\{\text{all the terms of Article VIII.}\right\}$$

X. The right-hand-side of the third equation appears to consist of the following terms:—

$$+\left[\left(\mathrm{e}-\frac{eq.\,c.\,f.}{2\,grav.}\right)\left(\frac{c}{a}\right)^2\right]\left(\frac{a'}{r'}\right)^4$$

$$\times\left\{\begin{array}{l}5\,(\sin\omega\cdot\cos\mathrm{l}.\sin v_{,}+\cos\omega\cdot\sin\mathrm{l})^2\times\sin\mathrm{l}-2.\sin\omega\cdot\cos\omega\cdot\cos\mathrm{l}.\sin v_{,}\\-\sin^2\omega.\sin\mathrm{l}-3\cdot\cos^2\omega\cdot\sin\mathrm{l}\end{array}\right\}$$

$$+\left[\frac{\sigma' a'^3}{(\epsilon'+\mu')\cdot A'^3}\cdot\frac{\epsilon+\mu}{\epsilon+\mu}\right]\left(\frac{A'}{R'}\right)^3\cdot\frac{r'}{a'}\cdot\left\{-\sin l\right\}$$

$$+\left[\frac{\sigma' a'^3}{(\epsilon'+\mu')A'^3}\cdot\frac{\epsilon^2-\mu^2}{(\epsilon+\mu)^2}\cdot\frac{a'}{A'}\right]\cdot\left(\frac{A'}{R'}\right)^4\cdot\left(\frac{r'}{a'}\right)^2\left\{+3\cdot\cos l\cdot\sin l\cdot\cos\overline{v'-V'}\right\}$$

$$+\left[\frac{\sigma' a'^3}{(\epsilon'+\mu')A'^3}\cdot\frac{\epsilon^3+\mu^3}{(\epsilon+\mu)^3}\cdot\left(\frac{a'}{A'}\right)^2\right]\cdot\left(\frac{A'}{R'}\right)^5\cdot\left(\frac{r'}{a'}\right)^3$$

$$\left\{-\frac{15}{2}\cos^2 l\cdot\sin l\cdot\cos^2\overline{v'-V'}+\frac{3}{2}\sin l\right\}$$

$$+\left[\frac{\sigma' a'^3}{(\epsilon'+\mu')A'^3}\cdot\frac{\epsilon+\mu}{\epsilon+\mu}\right]\left(\frac{A'}{R'}\right)^3\cdot\frac{r'}{a'}\cdot bt\left\{+3\cos l\cdot\cos\overline{v'-V'}\cdot\sin\overline{V'-K}\right\}$$

$$+\left[\frac{\sigma' a'^3}{(\epsilon'+\mu')A'^3}\cdot\frac{\epsilon^2-\mu^2}{(\epsilon+\mu)^2}\cdot\frac{a'}{A'}\right]\cdot\left(\frac{A'}{R'}\right)^4\cdot\frac{r'^2}{a'}\cdot bt$$

$$\times\left\{\begin{array}{l}-\frac{15}{2}\cos^2\lambda\cdot\cos^2\overline{v'-V'}\cdot\sin\overline{V'-K}\\ +\frac{3}{2}\sin\overline{f-V'}+3\sin^2 l\cdot\sin\overline{V'-K}\end{array}\right\}$$

All these algebraical expressions require revision, and may perhaps admit of some reduction.

XI. The nature of the considerations which will introduce some of the contingent variations may be judged partly from discussions similar to the following:—

Assuming that the length of the mean sidereal year is invariable, and is accurately known, $\frac{\sigma'}{A'^3}$ is invariable, and is a known quantity. But A′ is not certainly known; and, therefore, if A be its assumed value, we must write its real value in some such shape as this: $A'=A\left(1+(1)\right)$, the quantity (1) being a contingent correction. Then, as $\frac{\sigma'}{A'^3}$ is invariable, or $=\frac{\sigma}{A^3}$, $\sigma'=\sigma\frac{A'^3}{A^3}=\sigma\left(1+3\,(1)\right)$.

Now we cannot assume that the length of the mean sidereal month is invariable; moreover, our estimate of the masses $\epsilon+\mu$ may be liable to error. Therefore, a' ought to be expressed by $a\left(1+(2)\right)$, where a is the assumed value for 1900, and where (2) contains a constant value, and also a value depending on time. And $\epsilon'+\mu'$ must be represented by $\left(\epsilon+\mu\right)$

$\left(1+(3)\right)$, where (3) is constant. Therefore, $\dfrac{\sigma' a'^3}{(\epsilon'+\mu')A'^3}$ will become $\dfrac{\sigma a^3}{(\epsilon+\mu)A^3} \times \left(1+3(1)\right) \cdot \left(1+3(2)\right) \cdot \left(1-(3)\right) \cdot \left(1-3(1)\right) = \dfrac{\sigma a^3}{(\epsilon+\mu)A^3} \times \left(1+3(2)-(3)\right)$. And to form $\dfrac{\sigma' a'^3}{(\epsilon'+\mu')A'^3} \cdot \dfrac{a'}{A'}$, we must multiply the last expression by $\dfrac{a}{A}\left(1+(2)-(1)\right)$, and it becomes $\dfrac{\sigma a^3}{(\epsilon+\mu)A^3} \cdot \dfrac{a}{A} \times \left(1-(1)+4(2)-(3)\right)$.

No general rule can be given for the appropriation of contingent symbols to terms. But every principal coefficient (as the coefficients of elliptic equation, evection, variation, annual equation, inclination, evection in polar distance, and many much smaller coefficients,) must have its independent symbol of contingent correction.

The first part of the process of applying these principles to the Lunar Theory will consist in developing the expressions of Articles VIII., IX., X., by substitution in them of Delaunay's numerical values of $\dfrac{a}{r}$, v, l, and in ascertaining how nearly these values will satisfy the three equations of Article V.

I have developed, numerically, the expressions for all the requisite powers of $\dfrac{r}{a}$ and $\dfrac{a}{r}$, and I am proceeding with the development of sin l and various powers of cos l.

1874, *January* 9.

Additional Notes concerning Sir William Herschel's Double Stars. By S. W. Burnham, Esq.

(*Communicated by Mr. Dunkin.*)

I have given some attention, more particularly within the last few months, to the telescopic examination of Double Stars with reference to which there exists some uncertainty as to place, magnitude, position-angle, and distance, identity with other double stars, &c. There is a large number of such objects, several hundred perhaps, principally from the Catalogues of the two Herschels, which require or may require correction in some of these respects. Some have been found to be out of their

assigned places, and very many ascertained to be identical with stars heretofore regarded as distinct objects.

At the time of preparing the paper on "Errors and Omissions in the Catalogue of Sir William Herschel's Double Stars" (*Monthly Notices*, xxxiii. No. 9, Supplementary Number), I was not aware of a communication on the same subject by Sir John Herschel (*Monthly Notices*, xxviii. p. 151), so that a few of my corrections had already been noted.

The following, some of the results of an examination or search for the stars referred to, relate for the most part to their identity. In a few instances, as will be seen, a considerable error in place has been detected. All the places given from Herschel's Catalogue (*Memoirs of the R. A. S.*, xxxv.), or otherwise, are for 1880.—

H. I. 47.

No measures of distance: $P=336°\cdot8$. My estimates are: $P=330°$: $D=2\frac{1}{2}''$: Mags. 8, $8\frac{1}{2}$. The place of this star from the *Washington Transit Zones* (No. 9, Z. 205) makes Herschel's declination 7′ too large. After these observations, I found that this pair was identical with H. 5252 (*Cape Observations*), where $P=328°\cdot0$: $D=2''\cdot1$: Mags. 8, 8. The place from *Cape Observations* agrees exactly with that from the *Washington Zones.*

H. I. 60.

$P=286°\cdot8$. No measures of distance. Herschel II. says, "It is barely possible that this may be Σ 2441 with a change of 5° in position, but the place disagrees by 18′ in P. D." I have not been able to find any pair nearer than Σ 2441, which answers the description. H. 1366 has the same declination as H. I. 60, but is about 3^m *p*. This is a very faint pair, and not likely to be Herschel's, even if the position-angle corresponded. ($P=57°\cdot8$: $D=10''\pm$: Mags. 9·10, 12). About $2\frac{1}{2}^m$ *p*, the last is an 8 mag. star with a distant, very minute double companion. There is very little, if any, doubt of the identity of H. I. 60 with Σ 2441.

H. I. 77.

No measures of distance are given by Herschel. He makes $P=7°\cdot6$. I have looked up this pair, a very pretty double, and estimate $D=2''$: $P=10°$.

H. I. 90.

I have carefully examined the vicinity, and cannot find any double of Class I. in this place, and no pair nearer than S. 788 and Σ 2781.

H. III. 20.

$P=329°\cdot5$: $D=7''\cdot6$. "Identification very precarious from vagueness of description." Although the place given is 2^m *p*, and 24′ *n* of Σ 1838, they are undoubtedly the same. Struve gives: $P=333°\cdot8$: $D=9''\cdot04$.

H. III. 42.

No measures in Herschel. Σ 246 is 3^{m} *f* and $5'n$, and the only pair to be found in the immediate vicinity.

H. III. 52.

P=52°·9: D=13″·67. "Identification precarious; by bare possibility S. 465." This is certainly Σ 630 (=S. 465). Struve gives: P=49°·2: D=14″·0. Herschel's place should be diminished 25^{s} in R.A., and 28′ in Declination.

H. III. 103.

P=148°·4: D=12″·97. This is Σ 2446 (=P. xviii. 302) where P=154°·5: D=10″·13. There is a second minute companion, not mentioned by either, in the opposite direction, at a distance of 30″ or 40″. Herschel's place agrees substantially with Struve's.

H. IV. 34.

D=30″±. "Near 64 *Aquilæ*. No other indication of place." The only pair found is one in the field with *n p* 64 *Aquilæ*. P=190°: D=30″ or more: Mags. 9, 10.

H. IV. 127.

P=339°·9: D=16″·55. In the Catalogue this is called Piazzi xviii. 274 (that star being Σ 2434), and in his list of corrections Sir John Herschel states explicitly that H. IV. 127 is Σ 2434 (=Sh. 285), the place of which had been given in the first instance. Struve's measures of the wide pair of that star are: P=147°·0: D=25″·56. Suspecting from the discordance between the measures of Herschel and Struve, that these two stars were not the same, I carefully examined all the stars in the vicinity, and found beyond all question that Herschel's pair was identical with Σ 2447. Struve gives: P=334°·9: D=13″·82, differing but slightly from the measures of Sir W. Herschel quoted above. The place of H. IV. 127 should therefore be increased 3^{m} 48^{s} in R.A. and 39′ in Dec.

H. V. 31.

D=30″. "A choice between two stars, but the preceding the most probable." There are several small stars near this place answering the description in respect to distance, but the absence of the position-angle renders any absolute identification impossible.

H. V. 103.

"The place that of a star 8 m. in Argelander, very unequivocally indicated." This star is L. 35845, the place of which differs but slightly from Herschel's. It is also correctly given in *Positiones Mediæ*.

H. VI. 47.

R.A $=19^h\ 19^m{\cdot}9$; Dec. $=+1°35'$. No measures. "Identification very precarious." The only pair found is L. 36616, of which $P=105°$: $D=60''$: Mags. $7\frac{1}{2}$, $8\frac{1}{2}$. Reduced from Lalande, R.A. $=19^h\ 18^m\ 49^s$: Dec. $+1°36'$.

H. VI. 48.

R.A. $=19^h\ 20^m$: Dec. $=+1°20'$. No measures. "Identification very precarious." The only double found in the vicinity besides the last is L. 36659. $P=340°$: $D=25''$: Mags. 9, 11. The place from Lalande is: R.A. $=19^h\ 20^m\ 17^s$: Dec. $=+1°33'$.

H. VI. 49 and 50.

No measures given of these stars, which have the same approximate R.A. with a difference of but $5'$ in Dec. One of them must be P. xviii. 197, of which Smyth's measures are: $P=168°{\cdot}9$: $D=99''{\cdot}0$: Mags. 7, 9.

H. N. 10.

This star is entered, without measures, as Class III. with the position given as "preceding." Sir John Herschel has given no synonyms, and yet this pair is found in no less than four other double star Catalogues, two of which are by Sir John Herschel himself. The corresponding numbers are: H. N. 10 $=\Sigma$ C. P. 693 $=$ S. 763 $=$ H. 2996 $=$ H. 3002. South gave: $P=295°{\cdot}1$: $D=16''{\cdot}75$, and Herschel (2996) $P=291°{\cdot}6$: $D=15''\pm$. The places agree, with the exception of H. 3002, which is about 4^m preceding in R.A. There Herschel gives $P=120°\pm$: $D=25''\pm$, which he states were "estimated from a diagram." This will probably explain the reversing of the angle, and the increased distance. He calls it "a very fine double star," but gives no magnitudes. I have very carefully examined on several occasions all the stars within a considerable radius, and I am absolutely certain that these various Catalogues all refer to the same pair. There is something strange about the great difference in the estimated magnitudes by different observers. In the B.A.C. (7202) it is 6 m.; South, $7\frac{1}{2}$, 8; Herschel (2996) 9, 9+; in the *Washington Transit Zones*, where it is noted as "a beautiful double star," it is given 9 m. When examined recently the magnitudes appeared to be about as set down by South. In a low-power field $8'$ directly south I noticed a pretty $2''$ pair of very minute stars.

H. N. 18.

No measures in Herschel, Class II. A rather faint pair, estimated: $P=250°$: $D=5''$: Mags. 9, 9+.

H. N. 35.

No measures; angle "following." Herschel's place is:

R.A.$=23^h\ 31^m{\cdot}3$: Dec.$=-13°\ 45'$. This is *Aquarii* 355, and its place from Lalande (46271): R.A.$=23^h\ 31^m\ 27^s$: Dec. $=-13°\ 43'\ 34''$. Estimated, P$=90°$: D$=40''$: Mags. 7, 10.

H. N. 39.

R.A.$=21^h\ 14^m{\cdot}2$; Dec.$=+39°\ 13'$: P$=120°{\cdot}5$: D$=18''\pm$ "Place that of tho most probable star in Argelander." This is identical with No. 648 of Sir John Herschel's 7-ft. equatoreal measures (*Memoirs of the R. A. S.* xiii.), where it is marked "nova." There P$=114°$: D$=26''{\cdot}45$: Mags. 7, 11. I find that this is Weisse (2) xxi. 312, from which R.A.$=21^h\ 14^m\ 17^s$: Dec. $=+39°\ 15'$. It is about $1^h\ 20^s$ following Σ 2785.

H. N. 63.

R.A.$=14^h\ 55^m{\cdot}6$: D$=+54°\ 15'$. This is Σ C. P. 470; and the place, from *Positiones Mediæ*, R.A. $=14^h\ 56^m\ 0^s$: Dec.$=+54°\ 20'$.

H. N. 101.

Class III. No measures. Found and estimated, P$=330°$: D$=10''$: mags. 8½, 9.

H. N. 102.

P$=300$; Class III. In or very near this place is a pair of 10 m. stars. P$=300°$: D$=5''$. I failed to find any other double at all corresponding.

H. N. 110.

Class V. No measures. R.A. $=19^h\ 41^m\ 48^s$: Dec. $=+32°\ 47'$. Herschel's declination is too large, as this is S. 726, the place of which, from *Positiones Mediæ*, is:—R.A.$=19^h\ 41^m\ 59^s$: Dec.$=+32°\ 36'$.

H. N. 112.

No measures. The polar distance in the Catalogue is $19°\ 4'$, which, as corrected by Herschel, should be $99°\ 4'$. This makes it identical with Σ 3008 (= S. 829), the places agreeing very nearly. This pair is now a well-recognised binary. It is B.A.C. 8154.

H. N. 127.

The place given is the same as that of H. VI. 4 (α^2 *Capricorni*), while H. VI. 92, which is described as in or near the place of H. N. 127, is $30'$ north and 5^m preceding. Referring to Sir William Herschel's original Catalogue, I find this designated as "the middle one of 3, *n f α Capricorni.*"

H. N. 129.

R.A.$=18^h\ 57^m{\cdot}5$: Dec.$=-22°\ 54'{\cdot}9$. No measures; Class I. This is L. 35530, from which its place is:—R.A. $=18^h\ 56^m\ 0^s$: Dec.$=-23°\ 4'\ 31''$. My estimates of angle and distance are as follow:—P$=300°$: D$=8''$: Mags. 7½, 10.

H. N. 130.

Class I. I have not been able to find any double star of the first class in this place.

H. N. 131.

"Unidentifiable; Class III." R.A. = $21^h 49^m \pm$: Dec. = $-15° 6' \pm$. Although the difference in declination is considerable, I have no doubt, after a careful search, that this pair is the same as H. 3071, the place of which is:—R.A. = $21^h 51^m 14^s$: Dec. = $-15° 42'$. Herschel gives of this, P=$318°.5$: D=$18''$: Mags. 8, 11. "Fine." There is a $20''$ pair of 12 m. stars (H. 5522, *Cape Observations*) very near the place of H. N. 131; but from the faintness of both stars it is too insignificant an object to make it at all probable that it is the pair discovered by Herschel I.

H. N. 139.

R.A.=$21^h 12^m.1$: Dec.= $-15° 48'$. No measures; Class I. This place is also largely in error if the double found is the one in question. The only double of Class I. found after a careful search and examination of all the stars near, is L. 41483. It is a beautiful and interesting pair. My estimates of position and distance are:—P=$100°$: D=$1''.5$: Mags. 8, 10. Its place from Lalande is:—R.A=$21^h 15^m 39^s$: Dec.= $-15° 26'$.

H. N. 140.

R.A.= $22^h 37^m.1$: Dec. = $-5° 30'$. Class II. I have not examined this star. Struve, in *Positiones Mediæ*, gives its place:—R.A. = $22^h 35^m 51^s$: Dec. = $-5° 44'$, which is probably correct.

Chicago, U.S., 1873, *December* 8.

On Red Stars in Cygnus. By G. F. Chambers, Esq.

In Vol. xxxiv. of the *Monthly Notices*, p. 54, I observed some remarks by Mr. J. Birmingham concerning Red Stars catalogued by Schjellerup, on which I should like to say a few words, for Red Stars have long interested me, and I have paid much attention to them. It was on this account that I incorporated Schjellerup's list into my *Descriptive Astronomy*, making however some additions to it. After the publication of the Catalogue in that work, I resolved upon undertaking an examination on my own account of every star contained therein which was visible in England, with

the ultimate object of eliminating all stars which were beyond the reach of a 4-inch refractor, or otherwise were devoid of interest.

On reading Mr. Birmingham's paper, I turned to my annotated Catalogue of Red Stars, and there found two stars apparently identical with those alluded to by Mr. Birmingham, and by him designated, from Schjellerup, Nos. 251 and 252.

In the Catalogue in my book they are respectively numbered 264 and 265, and their places for 1870, and other particulars, are set down as follow:—

	R.A. h m s	Decl. ° ′	Mag.	Remarks.
264. Anon. Cygni	21 37 54	+37 25·3	8	Red.
265. Anon. Cygni	21 38 59	+37 16·1	8·5	Extremely intense ruby.

It was in 1869 that I began to examine all the Red Stars in the aforesaid Catalogue, and I ceased work in the summer of 1871. All the stars which I viewed were one by one marked in classes as follow:—

1. Conspicuously red, and altogether very interesting.
2. Tolerably red: worth looking at.
3. Reddish: not worth much.
4. More or less devoid of colour, and to be removed from the Catalogue in consequence.

In addition to the above classification, the remarks attached to each star were considered, and if thought desirable, revised.

From my MS. notes I find that at some period between the summer of 1869 and that of 1871, I looked for and found *both* the stars of which one is now understood to be missing. The column of remarks I find I did not alter, but I put my 264th star into the 2nd class, as above, and my 265th star (the missing one) into the 1st class. Both stars I have no doubt were viewed on the same evening, and probably in the order of R. A., that being my invariable custom in such matters.

What the value of this testimony may be I will not presume to assert, but having regard to the circumstances under which I worked, I do not think I could have fallen into a mistake.

The telescope employed was a 4-inch Cooke refractor, equatoreally mounted in a fixed observatory in Kent, where I was residing during the years in question.

Eastbourne, Sussex,
1874, *January* 8.

On a reported Occultation of Regulus by the planet Venus, A.D. 885, *September* 9. By J. R. Hind, Esq.

In the manuscript of the work of the Arabian astronomer Ibn-Jounis, described by Delambre, *Astronomie du moyen Age*, p. 76, *Regulus* is said to have been occulted by *Venus*, on a date which corresponds to A.D. 885, September 9, in the Julian Calendar. The observation is thus given at p. 87: "Occultation de *Régulus* par *Vénus*, le 9 Septembre 885. Une heure avant le lever du Soleil, chacun des deux jours précédens, le mouvement de *Vénus* avait été de plus de 1°."

I have examined this observation by means of M. Le Verrier's Tables of the Sun and Planets, and with the following results. I give my numbers in sufficient detail to admit of verification by any one curious enough to repeat the work:—

A. D. 885.	Paris M. T.	♀ Helioc. long.	♀ Helioc. lat.	♀ Rad. Vect.	⊖ True long.	⊖ Log. Rad. Vect.
	d h	° ' "	° ' "		° ' "	
Sept.	9 12	77 12 7	+0 37 32	0·7191606	351 23 14	0·0002061
,,	10 0	78 0 40	0 40 21	0·7191138	351 52 47	0·0001429
,,	10 12	78 49 12	+0 43 8	0·7190670	352 22 20	0·0000795

Hence the geocentric places of *Venus* are:

		True R. A.	True Decl.	Log. Distance.
	d h	° ' "	° ' "	
Sept.	9 12	136 53 30	+17 0 35	0·07509
,,	10 0	137 29 39	16 51 41	0·07626
,,	10 12	138 5 45	+16 42 38	0·07741

Aberration in R.A.	−29"·7	Diameter of *Venus*	14"·0
,, in Decl. . . .	+ 7·4	Horizontal Parallax of *Venus*	7·5

I take the position of *Regulus* from the last Greenwich Catalogue (1864), and its proper motions from M. Le Verrier's *Annales*, II. p. 198, and by the rigorous trigonometrical formulæ thus find for the beginning of A.D. 885:

Regulus Mean R. A. . 136° 59' 3"·7 Mean δ +17° 0' 58"·0

The reduction constants after Bessel for the date are:

log. A +1·2672 log. B −0·4860 log. C +9·5634 log. D −0·4312

and, therefore, the corrections for apparent place of star:

R. A. +2"·6 Decl. +1"·2

I assume the observation to have been made at Baghdad; and from the above numbers find the conjunction in Right Ascension would take place on September 9, at $16^h\ 50^m$ mean time, the planet being then $1'\ 47''$ south of the star—both geocentric. The nearest approach would occur about $16^h\ 43^m$, at which time the distance may be taken $1'{\cdot}7$. The Sun rose at Baghdad at $17^h\ 42^m$; and the hour before sunrise, when it would appear the observation was made, therefore corresponds to the time found for nearest approach.

At a distance of $1'{\cdot}7$ it may perhaps be granted that the star would be lost to the naked eye in the comparative blaze of the planet, and thus M. Le Verrier's Tables sufficiently represent the place of *Venus* one thousand years ago. It will be remarked, that this near approach took place when the planet was in nearly the same part of its orbit that it will occupy during the approaching transit.

As an addendum to the above, I give the results of my examination of another interesting observation recorded by the same Arabian astronomer (Delambre, *Astr. du moyen Age*, p. 80): "Conjonction de ♂ et ♀, 13 Février 864. ♀ et ♂ paraissaient à la vue se toucher au commencement de la nuit." I again make use of M. Le Verrier's Tables of the Sun and Planets, and find:

A. D. 864.	Paris M. T. d	h	☉ True long. ° ′ ″	☉ Log. Rad. Vect.
Feb.	13	0	148 43 32·9	9·9967588
„	13	6	148 58 33·2	9·9967883

	d	h	♀ Helioc. long. ° ′ ″	♀ Helioc. lat. ° ′ ″	♀ Log. Rad. Vect.
Feb.	13	0	52 44 21·2	−0 47 49·9	9·8578530
„	13	6	53 8 30·8	−0 46 26·6	9·8578329

	d	h	♂ Helioc. long. ° ′ ″	♂ Helioc. lat. ° ′ ″	♂ Log. Rad. Vect.
Feb.	13	0	24 50 5·3	−0 30 25·3	0·1654763
„	13	6	24 58 35·3	−0 30 9·0	0·1655668

And hence the geocentric places:

	d	h	♀ Long. ° ′ ″	♀ Lat. ° ′ ″	♂ Long. ° ′ ″	♂ Lat. ° ′ ″
Feb.	13	0	2 36 9	−0 26 49	2 37 1	−0 20 26
„	13	6	2 54 17	−0 26 4	2 48 6	−0 20 15

The conjunction in Longitude would therefore occur February $13^d\ 3^h\ 32^m$ mean time at Baghdad, when *Mars* would be north of *Venus* $6'{\cdot}3$, which is a degree of approximation not inaptly represented by the words of the Arabian astronomer, as regards naked-eye observation.

On the present Dimensions of the White Spot Linné. By C. E. Burton, Esq.

On the 4th of June 1873, the length of the major axis of the white cloud was measured by my friend Mr. Erck, and the mean of the measures was found to be 4″. Instrument Alvan Clark's 7½-inch. Power 200. The terminator passed at the time close to the eastern walls of *Copernicus* and *Clavius*. On the same night I made a sketch of *Linné* with my own 12-inch reflector, using powers of 238 and 408.*

During the next lunation another sketch was obtained with the same instrument on July 15 at 15^h, approximate G. M. T., at which time the terminator had just passed *Bessel*. On this occasion no trace of the white spot was seen, though the definition was very fine, and it was carefully looked for. The small crater of *Linné* was beautifully shown. Instrument used, the 12-inch equatoreal reflector; powers 238, 514, and 940; all measured, and all single lenses.

1873, December 27 at $6^h\ 15^m \pm$ G. M. T. Measured the major diameter of the white spot. Mean of six measures 5″·5. Extreme difference of measures 0″·98. Terminator touching east wall of *Eratosthenes* and west wall of *Clavius*. The shadow thrown by the west wall of the small crater internally was well seen at times with a positive power of about 240. No visible shadow from east wall of craterlet.

1873, December 31. Time not recorded, but about 9^h G. M.T. The major axis of the white spot was measured with the position-wire-micrometer; power 240. 10 measures. Mean 5″·45. Extreme difference 0″·82. Three measures of minor axis taken G. D. 0″·48. Mean 3″·4.

1874, January 2 at $11^h \pm$ G. M. T. Four measures. Mean 5″·3. G. D. 1″. Power 240. One measure with 120 gave 5″·4 for the same dimension.

1874, January 6 at $12^h\ 30^m \pm$ G. M. T. Definition indifferent, and wind troublesome. Mean of five measures 4″·8. Power 240. Terminator close to west boundary of the *Mare Crisium*.

On December 31, six additional measures of the major axis of the white spot were taken with power 400, with which the edges of the spot appeared extremely nebulous.

Similarly, on January 2, 400 gave the spot a diameter of 4″, the measures being accompanied by the note, that the whiteness was very ill-terminated.

High Illumination.—*Linné* appears under good definition as a white spot, the boundary of which is tolerably distinct, surrounded by a nebulous chevelure, of varying width, which fades rapidly into the surrounding tint. When definition is not at the best, and the power used is high, the inner white spot will be measured,

* The two sketches made by Mr. Burton were exhibited at the meeting.

the terminal nebulosity not attracting attention. This probably is the explanation of the comparatively small diameters given by power 400.

On tabulating the observations of the white spot made by Dr. Huggins, Professor Tacchini, Mr. Erck, and myself, we have:

Date.	Diameter.	Observer.	
1867, July 9	7″·85	Dr. Huggins.	Moon 8 days old.
1867,	7 ·94	Prof. Tacchini	
1873, June 4	4 ·0	Mr. Erck.	
1873, Dec. & Jan.	5 ·4	B.	Mean of the three best results with the 12-in. reflector.

Is not the white spot in a state of change?

Loughlinstown, Co. Dublin,
1874, *January* 7.

Note on the Lunar Crater Linné. By William Huggins, Esq., D.C.L., LL.D., F.R.S.

The different aspects which this crater assumes under different conditions of illumination have raised the question whether any permanent change can be ascertained to have taken place in it.

In 1867, I presented to the Society a Note on the appearance of this object, and some micrometrical measures of the Bright Spot and of the small Crater. (*Monthly Notices*, vol. xxvii. p. 296.) In that Note I referred to some early observations of the object which appeared to me to be in favour of the unaltered state of the crater from the time of Schroter (1788) to that of the observations of 1867.

I venture now to present some recent observations and measures of *Linné*, as possibly of some value in reference to the question, whether during the last eight years we have any indication of permanent change.

The White Spot.—Under some conditions of illumination, the shallow saucer-like form of the large crater is seen distinctly; at other times the surface of the White Spot is similar in appearence to a cloud, presenting no distinct details and remaining undefined when the small neighbouring craters are seen with distinctness.

This cloud-like appearance is strong on the south-east and north-west sides of the shallow crater; and when the form of this

crater is not seen, this cloud-like appearance is so marked on these sides as to change the apparent direction of the oval form of the spot. At these times, the long axis of the oval appears to be in the direction from E.S.E. to W.N.W., but the true direction of the longer axis of the shallow crater is from S.E. to N.W.

November 5, 1867, I found this long axis to be inclined to a parallel of declination at an angle of 55°.

Two measures, on December 27, 1873, gave an angle of 48°. This difference is not greater than may be due to the circumstance that, in trying to place the wire of the micrometer in the direction of the longer axis, the judgment is probably influenced by the side of the crater which is seen with greater distinctness, and as the object is not a regularly formed oval, it seems not improbable that some discrepancy may present itself from this cause between measures taken at different times.

In my former Note on *Linné* I gave the following measures of the Bright Spot:—

July 9, 1867	Length of Spot	7″·85
	Breadth „	6 ·14

The crater was measured again on

November 5, 1867	Length	7″·58
	Breadth	5· 95
December 27, 1873	Length	7· 16
	Breadth	4· 3

I would remark, that in the breadth of crater, that is the direction from S.E. to N.W., the measures are more likely to vary, because, as I have already remarked, it is on these sides that the outline of the crater is more interrupted by the cloud-like appearance of the surface, and changes more quickly by differences of illumination. At the time of making these measures, and almost before completing them, I lost the distinct vision of the outline of the shallow crater in the white cloud-like appearance which came over the spot.

Small Crater.—The words I used in my Note of 1867 describe exactly the appearance of this object on December 27, 1873: "In the centre of *Linné*, but rather nearer to the western margin, was seen the small crater. This object was well defined in the telescope. The interior of the small crater was in shadow, with the exception of a small part of it towards the east." "The small crater, which appears to be deep, has a narrow margin brighter than the White Spot on which it falls. The measures of the crater include the narrow bright margin."

In 1867, I measured the small crater across in several directions, and then took the mean of the measures on the sup-

position that the small crater was sensibly round. The result was: Diameter of small crater, $1''\cdot71$.

In my recent measures, I took one set in the direction of the longer axis of the large crater, and a second set at right angles to that direction. These measures seem to show that the small crater did not appear quite round at this time, but was sensibly oval in a direction at right angles to the larger crater. I give all the readings of the micrometer, that a better estimate may be formed of the value to be given to the measures.

First set, in the direction of the longer axis of the shallow crater:

21·5
21·
21·
—— mean 21· = $1''\cdot73$

Second set, at right angles to the former direction:

22·5
22·7
23·
—— mean 22·7 = $1''\cdot98$

The mean of these, $1''\cdot8$, agrees almost exactly with the measures of 1867.

The occasional observations I have made of this object during nine years do not appear to me to afford any evidence of a permanent change of the lunar surface at this spot. The different aspects which this object assumes under different conditions of illumination appear to me to be exactly the same as those which it presented in 1867.

The measures in 1867 were made with a refractor of 8 inches aperture and an eye-piece magnifying 500 diameters. The observations were made in 1873, with a refractor of 15 inches aperture and an eye-piece giving a power of 420 diameters.

I append a few extracts from my note-book of some of the appearances which this object presents:—

1866, December 14.—With Savart's polariscope. The bands passed unbroken across *Linné*, which appeared as a white spot.

1867, July 8, 7^h 10^m.—As a shallow oval crater, a little shadow thrown by the western margin.

Ditto, ditto, 8^h to 8^h 30^m.—Small cone as a little hill a little west of centre. Shadow cast by it pointed, and extending nearly to the eastern margin.

July 9, 8^h 30^m to 9^h 30^m.—The boundary of white spot gradually passes into surrounding surface, more irregular at some points. Measures will differ as more or less of the faint outline is included, and according to the points of the outline selected.

November 6.—Several outlying portions of white surface of a smaller degree of brilliancy in contiguity, if not physically connected, with *Linné*. The boundary especially uncertain from this cause at the western end and also along the northern side. The southern side is less interrupted, and here the bright surface forming *Linné* terminates more abruptly. There are other spots on the Moon which possess a similar want of distinctness.

November 11 (One day short of full Moon).—Under this illumination the small crater, at least its rim, is much brighter than the flat crater on which it occurs. The peculiarity of its appearance at this time consists in the rapid diminution of brightness from the centre outwards.

December 4.—An offshoot of bright surface from the bright spot towards the S.W. was very distinct.

1868, March 30.—Nearly as sharply defined as neighbouring craters, not the usual cloudy appearance. Ring wall high, especially on the western side. Curious effect of eye retaining a defined object. The small crater appears, by motion of air, to swing to and fro over the white patch.

1873, July 4, 8^h to 9^h 30^m.—*Linné* quite nebulous at edges, no regular outline, but broken up by small projections. The oval appeared in the direction of E.S.E. to W.N.W.

August 1.—The direction of the longer axis seems to change while I am looking, sometimes E.S.E. to W.N.W., at others from S.W. to N.E. (the true direction of the shallow crater); the former direction (at right angles to the true one) appears more persistent.

December 27, 6^h 30^m to 7^h.—At first oval crater very distinct. Half an hour later I lost the outline of this in a bright nebulous patch. This nebulous light more marked on the longer sides of the oval, so that, as on former occasions, the oval form of the patch seemed sometimes in one direction and sometimes in the opposite direction. The small crater seemed to vary in position on the white patch, as in former years, probably from the varying visibility (from atmospheric changes) of the outside portions of the large crater.

On the Colour and Brightness of Stars as measured with a new Photometer. By W. H. M. Christie, Esq., M.A., Fellow of Trinity College, Cambridge.

Various plans have been proposed for referring the colours of stars to a standard scale, but most of them depend on the comparison of the star's light with painted surfaces, which, being necessarily viewed by artificial light, are liable to play the observer false tricks, especially where any colour containing green is concerned. Besides this, the eye is so readily biassed by any coloured light previously seen, that no reliable results are to be expected, unless the star and the comparison light are placed side by side. This is effected in Zöllner's photometer; but the comparison of an artificial star with the interference image of a star as seen in a telescope can hardly be considered satisfactory, at any rate in determining colour, and the use of the tints of polarization is open to the two-fold objection, that it must be difficult to match any given *hue*, and nearly impracticable to express the result in terms of the

three primary colours. As every hue in nature is compounded of white and some colour of the spectrum, or which is the same thing, of the three primary colours, any given hue must be a function of two quantities, and therefore cannot be expressed in terms of a single circle reading, as Zöllner appears to have attempted. In fact, we may express both the intensity and hue of any object by the quantities of red, green, and blue in it, or, if we prefer it, by the quantity of white, together with the quantity and position of some colour in the spectrum.

The instrument which I am about to describe is on the principle of Clerk-Maxwell's Colour-box (described in the *Philosophical Transactions*, 1860), in which definite quantities of red, green, and blue light can be compounded so as to match any given hue.

The objects which I have sought to attain are:

1. To place two uniform surfaces of sensible area side by side and in contact for comparison.
2. To determine both the intensity and hue of the light coming from a star or other body.
3. To obtain a fixed standard of whiteness.
4. To compare the colours of different portions of the disks of the Moon and planets.

It being premised that the photometer is attached to the eye-end of an equatoreal by means of a ring clamp, the general arrangement is as follows:—

The beam of parallel rays, of which the light of a star consists, after passing through the object-glass and eye-piece of the equatoreal, falls on a lens L, at the principal focus of which the eye is placed, so that a round spot of light is seen on one half of the lens, whilst the other half receives the compound comparison light, which will, if a suitable diaphragm be placed in front of the lens, be seen as a round spot in contact with that formed by the star, and exactly of the same size. The diameter of the spot is of course the ratio of the aperture to the magnifying power. The compound light falls on L after having passed through the collimating lens C and prisms P, from three slits, R, G, B, opposite which are the three flames of a paraffin lamp M, of peculiar construction.* The slits R, G, B are so adjusted, that of the light coming from R, and forming a spectrum at the eye-end, the slit E (behind which the eye is placed) will receive red

* This arrangement, in fact, constitutes a spectroscope in which three spectra are superposed at the eye-end, but it must be remembered that no eye-piece is used, and that a coloured field, not a spectrum, is seen by the eye at E. The dispersion is given by two flint prisms of 60° (forming a spectrum 4·5 inches in length from A to H at the lamp-end), but for convenience the spectrum is turned through 90° by the use of a right-angled reflecting prism. In any photometric enquiry, simple prisms are much to be preferred to any form of direct vision prism, as with the former suitable stops can be placed to cut off false spectra. The focal lengths of the lenses C and L are 14 and 6 inches respectively.

rays of a certain refrangibility, whilst it will receive green rays from G, and blue rays from B; the resultant light will therefore be a compound of red, green, and blue, the quantities of each being measured by the products of the width of the slit E into the areas of the slits R, G, and B, respectively.

The first thing, then, is to have the means of varying these areas without removing the eye from E. Without entering into detail, it may be sufficient to explain that the slits slide on a dovetail, and are closed by spiral springs, whilst they are opened by the rods D, E, F, which terminate in a screw and a taper conical head. As the rod is turned, the cone is driven between the jaws of the slit, which thus opens equally on both sides, so that the colour of the light remains unaltered. A horizontal slit, which can, before the observation, be adjusted to any convenient width (not necessarily the same for the three colours) is placed in front of all; the width of the eye-slit is not usually altered unless for stars widely differing in brightness. Having carefully matched the light of any given star by turning the milled heads D, E, F, we have next to measure the areas of the slits R, G, B as accurately as possible. A convenient way of doing this is, after removing the lamp, to place an ordinary scale dynamometer in the channels H, I, K successively, these being so adjusted that the light from a gas jet reflected up the collimator by a tin reflector placed outside C will enter the dynamometer after passing through the slit; both the width and height of the slit can thus be read off by estimation to $\frac{1}{3000}$ inch, the product of the numbers giving the area required. I have also used a graduated steel wedge, but prefer the dynamometer, as not likely to disturb the slits in any way.

The lamp M, on which everything depends, has three ordinary paraffin burners soldered side by side, so as to be opposite the three slits; the chimney is rectangular and of tin, with two narrow horizontal openings covered with glass, one in front to illumine the slits, the other behind to verify the height of the flames at any time.* Owing to the strong draught necessary in a paraffin lamp, it occurred to me that the lamp might be considerably inclined without affecting its burning, and such I have found to be the case; the flame goes straight up the chimney, and remains free from smoke even when the lamp is inclined 60° or 70°, so that by fixing it at an angle of 25° or 30° to the plane of the slits we can work up to the zenith. That objectionable article, a swinging lamp, is thus got rid of; and though there may be a little difficulty in managing three flames, they are, I am convinced, less troublesome than any arrangement for reflecting the light from a single flame down the collimator. The performance of the lamp is very satisfactory, and its light remarkably constant and pure.

* This can also be done conveniently by viewing the light from each slit in succession through an eye-piece at E, where it will be easy to see when the white part of the flame falls on the slit.

I will now pass to the third point, a fixed standard; and it will be necessary to discuss this question at some length, though I do not propose in the present paper to enter on the subject of colour in general, but only in its bearing on the constitution of the heavenly bodies. My first object being to measure the light of stars, I have adopted provisionally the positions of the three standard colours which Clerk-Maxwell gives in the *Philosophical Transactions*, 1860, viz.: Red, wave length (in Fraunhofer's measure) 2328; Green, wave length 1914; and Blue, wave length 1717. Now it appears from Clerk-Maxwell's observations that any colour of the spectrum can be matched by a mixture of any two of these three, but that to match white light we require a mixture of all three, in certain definite proportions. It becomes then a matter of some interest to settle what these proportions are. White light may be taken as that which contains an equal admixture of all the rays of the spectrum, or in other words, which has the same energy for equal areas in all parts of the diffraction spectrum, but in the present state of our knowledge of the relative intensities of the colour sensations this definition has no practical value, and we must be content with assuming some convenient standard, all that is necessary being that it should agree tolerably well with the ordinary notions of white light, and should be universally accessible and not liable to change.

There must evidently be something arbitrary in this selection; but provided our measures are all reduced on one system, it will be easy afterwards to refer them to the absolute standard, should it afterwards be found practicable to determine it.

The only method of eliminating all instrumental influences (such as absorption in the object-glass and eye-piece), is clearly to choose some heavenly body which will be affected in the same degree by these sources of error. The Moon would be the most natural standard, but here we are met by the objection of possible change and the variety of tint introduces a practical difficulty, though by selecting such an object as *Aristarchus* we may get at any rate a provisional standard to be afterwards compared with the ultimate standard. For the latter I would suggest the planet *Venus*, though I have not obtained any measures yet, owing to the present unfavourable position of this planet.

The average of a number of stars would answer the same purpose, and might further serve as the base of a scale of magnitudes, with this advantage over Sir J. Herschel's proposal of *α Centauri* (which, by the way, is not universally accessible), that it is in the highest degree improbable that the average of a number of stars in all parts of the heavens should change appreciably. Considering that observers were more likely to agree in their estimate of 6th magnitude stars, I have selected a number of such stars in which Piazzi, Argelander and Heis agree, thereby giving a great security against change: these should be used as clock stars are in determination of right ascensions, several of

them being observed in connection with other objects. Taking the mean of these as a standard for 6-magnitude, and Pogson's value of the Light ratio R=2·512, we should have a consistent scale, agreeing well on the whole with Sir W. Herschel's determination of the first six magnitudes, and also with the scale adopted by modern observers. The following table expresses the light of a star of any magnitude in terms of that of a 6-magnitude star:—

Magnitude	1	2	3	4	5	6	7	8
Light ratio	100·000	39·810	15·850	6·310	2·512	1·000	0·398	0·159

Since 5 Log R=2 the values for the lower magnitudes are given by shifting the decimal point two places in the value for the magnitude which is 5 higher in the scale.

But to express the light of a star, as measured with the photometer, in this scale of magnitudes it is necessary to have more observations of the relative intensities of the red, green, and blue, than I have yet been able to obtain. From a mean of 7 comparisons of these in succession with a constant white light (paraffin) it appears that in my instrument 5·9 parts of Red are equivalent in brightness to 14·6 parts of Green, or to 100 parts of Blue. These are expressed in terms of the breadth of the slits in the dispersion spectrum, where the red rays are crowded together and the blue spread out so that the unit is different for rays of different refrangibility; in order to refer them to the diffraction spectrum we must lay down the curve of which the abscissæ are scale readings of the spectroscope and the ordinates wave lengths, the tangent of the inclination of the tangent at any point of this curve will then be the factor for converting the breadth of the slit at that point. For the three standard colours, the factors are 1·84, 0·86, and 0·48, in my instrument. The intensities of the Red, Green, and Blue, in the diffraction spectrum would thus be roughly as 1 : 1 : 4.

It remains to explain how the positions of the principal Fraunhofer lines are determined. An eye-piece being applied at the eye-end, and the eye-slit (which is on a slide) placed in a definite position near the line of collimation, one of the slits at the lamp-end is moved till the line to be measured is brought into the centre of the slit; the scale at the lamp-end (which is divided to $\frac{1}{50}$ in.) is then read off. In practice, it has been found convenient to set the three slits to definite positions, and to move the eye-slit till the sodium lines in the light of the red slit appear, and then, by means of a scale at the eye-end, to set the eye-slit to its proper position.

It might seem that by comparing two similar surfaces, personality would be entirely eliminated, but this is not the case if there be anything like selective absorption in the eye of the observer. In his able paper, previously cited, Clerk-Maxwell points out that there is in most eyes in a greater or less degree

such an absorption of the rays between the Fraunhofer lines E and F, but confined apparently to the yellow spot of Sœmmering.

With regard to the isolation of a portion of the Moon's or a planet's disk, it may be remarked that, as an image (which may be viewed by an eye-piece) is formed at the eye-slit, the latter may be made use of for this purpose; I have, however, preferred the use of a pinhole at the principal focus of the equatoreal, as enabling the observer to examine a smaller portion of the surface; the diameter of my smallest pinhole is 6″, and it would not be difficult to make one of half that size. For double stars, however, the former method has generally been used.

Though the instrument (which was made for me by Mr. Browning) was received at the end of September 1872, I have not yet been able to accumulate sufficient observations for any certain conclusions, partly owing to the experiments necessary in an inquiry of this kind, but, in a far greater degree, to the very unfavourable weather of the past year. As it is, I have felt constrained to make use of some nights which were hardly suitable for delicate observations. The photometer was attached to the Great Equatoreal of the Royal Observatory, Greenwich, and the subjoined observations were made with it in the last few months; the earlier measures are reserved for future discussion.

Date. 1873.	Object.	Z.D. °	No. of Obs.	Areas of Slits. Red.	Green.	Blue.	Relative Intensities. Red.	Green.	Blue.	Apparent Brightness.	Corrected Brightness.
Aug. 11	☽ Aristarchus	60	1	676	2960	5110	1150	2030	511	3690	4610
Sept. 26	95 Herculis B	55	1	34·6	140	470	58·8	95·9	47·0	202	236
"	" A	58	1	33·1	140	373	56·3	95·9	37·3	190	230
"	β′ Lyræ	49	1	83·2	485	1010	141	332	101	574	631
"	β Cygni A	52	1	132	481	46·8	225	329	4·7	559	632
"	" B	58	1	22·9	55·8	173	38·9	38·2	17·3	94·4	114
"	η Pegasi	38	2	126	756	857	214	518	85·7	818	851
Sept. 27	95 Herculis B	53	2	15·9	99·2	64·3	27·0	68·0	6·4	101	115
"	" A	55	2	15·9	118	211	27·0	80·8	21·1	129	151
"	β′ Lyræ	47	4	59·0	445	660	100	305	66·0	471	508
"	γ Lyræ	52	2	64·3	551	834	109	377	83·4	569	643
"	χ^1 Cygni	57	2	14·7	105	117	25·0	71·9	11·7	109	130
"	χ^2 Cygni	64	2	23·2	87·5	9·4	39·4	59·9	0·9	100	135
Oct. 8	β′ Lyræ	46	2	42·8	256	484	72·8	175	48·4	296	317
"	γ Lyræ	45	2	59·9	410	739	102	281	73·9	457	489
Oct. 16	β′ Lyræ	48	2	75·6	704	828	129	482	82·8	694	763
"	γ Lyræ	55	1	82·0	767	899	139	525	89·9	754	882
"	η Pegasi	42	2	137	657	322	233	450	32·2	715	751
Oct. 29	β′ Lyræ	44	2	94·0	481	325	160	330	32·5	523	554
"	γ Lyræ	47	3	89·0	557	495	151	382	49·5	583	629

Date 1873.	Object.	Z.D. °	No. of Obs.	Areas of Slits. Red.	Green.	Blue.	Relative Intensities. Red.	Green.	Blue.	Apparent Brightness.	Corrected Brightness.
Nov. 1	Schjell. 241	55	2	9·1	46·2	133	15·5	31·6	13·3	60·4	70·7
"	β' Lyræ	61	3	60·0	661	655	102	453	65·5	621	789
Nov. 3	β' Lyræ	53	3	69·4	491	441	118	336	44·1	498	568
"	γ Lyræ	58	2	91·1	656	762	155	449	76·2	680	823
"	Schjell. 241	69	2	13·8	43·8	0·6	23·5	30·0	0·1	53·6	80·9
"	41 Pegasi *	49	1	9·0	43·5	56·4	15·3	29·8	5·6	50·7	55·8
"	40 Pegasi *	51	1	15·3	66·1	2·4	26·0	45·3	0·2	71·5	80·1
"	45 Pegasi *	52	1	14·4	60·3	0·0	24·5	41·3	0·0	65·8	74·4
"	52 Pegasi *	58	1	9·5	63·8	0·0	16·1	43·7	0·0	59·8	72·4
"	η Pegasi	53	2	238	1500	1470	405	1030	147	1580	1800
"	β Pegasi	54	1	290	1910	808	493	1310	80·8	1880	2160
"	☽ Mare Imbrium	45	1	205	1770	3470	349	1210	347	1910	2040
Nov. 12	Schjell. 241	64	3	8·1	32·4	0·0	13·8	22·2	0·0	36·0	48·6
"	40 Pegasi *	44	1	10·4	51·8	0·0	17·7	35·5	0·0	53·2	56·4
"	41 Pegasi *	46	1	5·7	33·8	78·4	9·7	23·2	7·8	40·7	43·5
"	η Pegasi	44	2	129	1150	468	219	788	46·8	1050	1110
"	β Pegasi	50	2	278	1320	26	473	904	2·6	1380	1530
"	60 Pegasi *	59	1	6·5	39·5	14·0	11·1	27·1	1·4	39.6	48·7
"	n^1 Orionis *	52	1	7·5	130	98·0	12·8	89·1	9·8	112	127
"	n^2 Orionis *	51	1	9·0	129	14·0	15·3	88·4	1·4	105	118
Nov. 30	η Pegasi	53	1	97·5	623	268	166	427	26·8	620	707
"	β Pegasi	60	1	166	847	258	282	580	25·8	888	1110
"	λ Tauri	42	2	65·3	425	466	111	291	46·6	449	471
"	n^1 Orionis *	50	1	13·6	101	112	23·1	69·2	11·2	104	114
"	n^2 Orionis *	50	1	12·8	101	32·7	21·8	69·2	3·3	94·3	105
Dec. 31	η Pegasi	63	3	117	710	293	199	486	29·3	714	942
"	λ Tauri	47	2	94·4	497	597	160	340	59·7	560	599
"	n^1 Orionis *	53	2	9·8	80·8	146	16·7	55·3	14·6	86·6	98·7

* Standard 6th magnitude stars:

Aug. 11. Hazy clouds; brightness of moon variable; many attempts made to get a good match. Diameter of Pinhole, 8″·6.
Sept. 26. East wind; images bad, and brightness variable.
Sept. 27. Definition good at times; south wind.
Oct. 8. Sky hazy, and definition variable. Bright moonlight.
Oct. 29. Rather foggy; images not good. Moonlight.
Nov. 3. Images fairly good. Diameter of Pinhole (for Moon), 8″·6.
Nov. 12. East wind; images bad, generally granulated, with rare fits of fair definition; colours of stars seemed variable.
Nov. 30. Sky hazy, except for n^1 and n^2 *Orionis*. Moonlight.
Dec. 31. Brightness and colour of stars very variable.

The numbers given in the columns headed Areas of Slits express the areas of the several slits in square $\frac{1}{500}$ inch, all the measures being referred to the same width of eye-slit ($\frac{1}{50}$ inch), and three significant figures only being retained; the same power (130) was used throughout on the equatoreal, though with very bright stars there is an advantage in using a lower power, whilst for stars below 6 mag. a high power which concentrates their light is perhaps better.

The next three columns are formed by multiplying the three preceding columns by $\frac{10}{5\cdot9}=1\cdot70$, $\frac{10}{14\cdot6}=\cdot685$, and $\frac{10}{100}=0\cdot1$, and express the intensities of the three colours in each case; the sum of these numbers is taken in the next column, which gives the apparent brightness subject to a correction for atmospheric absorption. This correction (taken from Seidel's Table) is applied in the last column, which therefore expresses the brightness of a star as it would be seen at the zenith. There is very little doubt that a different correction ought to be applied for each colour, so that the present reduction must be regarded as provisional, but it will serve to illustrate the method which may be advantageously employed. I have not thought it worth while at present to reduce these observations to the scale of magnitudes, which I have proposed. This will be done more advantageously when more observations have been collected, for it is evident that even with the selected 6th magnitude stars, there is no such approach to equality as we have a right to demand. There can be no doubt, for instance, that both n^1 and n^2 *Orionis* are now nearly twice as bright as an average 6-magnitude star, and yet we find Piazzi, Argelander, and Heis agreeing to one-sixth of a magnitude, and all three calling them 6·0 magnitude.

As might be expected, in observations of such a nature with a new instrument, there are certain anomalies in these measures which are probably due in part to scintillation, the effect of which is to make the star's light change from orange or yellow to blue-purple; this was remarked very distinctly on December 31. Another cause of error which, however, affects the brightness rather than the colour of stars, is the dryness of the air: with an east wind the spot of light usually becomes granulated, and there is in consequence a strong tendency to over-estimate its brightness. From a comparison of the discordance of the separate results for the same star on any one night, the mean error of a single measure appears to be about 10 per cent., equivalent to 0·1 of a magnitude. But after due allowance has been made for all probable sources of error, there remain discordances which seem to imply rapid changes of colour in some of the stars examined. Now change of colour shows a physical change, for it

is extremely improbable that such a pheuomenon should be caused by rotation. We know that as the temperature of a solid is raised, the colour changes from red to white, radiations of all wave lengths increasing, but the blue more than the red; now, we see something similar to this in the results for η *Pegasi*, β *Pegasi*, and Schjellerup 241; but I do not wish to insist strongly on the few measures I have obtained as yet, though there can be no doubt that η *Pegasi* is a new variable star of moderate period. Owing to unfavourable weather, the observations are for the most part separated by such long intervals as to give but little information on the light curves, and from the same cause I have been baffled in my attempts at observing a minimum of β *Lyræ* or *Algol*; the observation of λ *Tauri* on November 30, at $12^h\ 25^m$ G. M. T., was close to the minimum. If changes in the colours of stars are due to gases, as would seem probable, the law of radiation, given above for solids, will have to be modified; thus, an increase of temperature (in the case of hydrogen) would, I fancy, increase the red and blue without affecting the green. But to get further information on this point, we require measures of the colours of gaseous bodies under various conditions of temperature and pressure. In this way the photometer will supplement the spectroscope, for though it cannot give us the same definite information on the constituent elements of the heavenly bodies, it may tell us something about changes of temperature and pressure, and may be readily applied to the fainter orders of stars, a field left untouched by the spectroscope. Even with the brighter stars, much may be done by the help of a good photometer with which the *absolute* amounts of the three standard colours can be measured, for among the numerous lines of stellar spectra it must be very difficult to trace changes, but a change in colour will at any rate guide the search; thus, if the red and green increase together, a bright line has probably appeared in the intermediate yellow, or a dark line has faded out; again, if the green and blue decrease together, a bright line has probably faded out in the region near F.

A few words, in conclusion, on another application of this instrument. Recent researches have shown the importance of determining the changes in the relative brightness of the lines in the spectra of gases with changes of temperature; now, though I have not tried the experiment, yet I conceive there would be no difficulty in combining the photometer with a spectroscope so as to match the different bright lines, which might be done either by placing the lens L at the principal focus of the spectroscope, or by the use of an eye-piece as in measures of stars.

And now, having pointed out the results which we may hope for from such measures, I would earnestly commend this branch of astronomy to all those who have equatoreals, and do not know exactly what useful work to take up with them. With a very moderate expenditure of time and money they may do valuable work in sidereal photometry with the promise of a rich harvest of discoveries.

My object in this paper has been not so much to give results as to explain the method of using such an instrument, and, if possible, to induce others to take up a most interesting inquiry; this must be my apology for having, in some cases, drawn conclusions from very insufficient data, instead of waiting till further materials were accumulated, though I trust that such statements as I have ventured on will be regarded merely as working hypotheses intended to suggest new lines of research.

Blackheath,
1874, *January* 8.

Note on an Ancient Astrolabe. By R. J. Lecky, Esq.

This Astrolabe was found about the year 1846, under a large flat stone, on the mountain side opposite the northern entrance of Valencia Harbour, County of Kerry, Ireland. It was in a leather case, which went to pieces on being handled. Bewicke Blackburn, Esq. C.E., who was then a resident on the Island of Valencia, purchased it, and gave it to me a short time afterwards.

About six months after this, I was again at Valencia, and accompanied by Mr. Blackburn, took the lad who had found it to the place; and, on searching amongst the rocks, found a piece of the old case, thus confirming the lad's report.

The instrument is of brass, weighs 5lbs. $9\frac{3}{4}$ oz. and is 7 inches diameter by over $\frac{5}{8}$ths of an inch in thickness; with a strong outer circle, the lower part being cast solid in order to give it weight, and at the upper part is a double or cross joint, with a ring for suspension. The vanes are $1\frac{1}{4}$ inch square, with very minute sight-holes just large enough to admit a very fine needle; they are cast solid on to the cross arm, which is connected to the body of the instrument by a turned pin with a neatly cut rose head and ornamental forelock: this arm ends with a pointer at each end, the fiducial edges of which range with the sight-holes. The upper half of the circle is alone divided into degrees; and these are not numbered, neither is there a name or date on the instrument. It has evidently been left unfinished; but all the work, as far as it has gone, has been carefully executed: the double joint, cross arm centre pin, and vanes have been thoroughly well made and fitted. It is impossible now to say where this Astrolabe was made, or where it may have originally come from; but, judging from the age in which they were used, and the style of the workmanship, the idea that it belonged to the Spanish Armada seems probable, and that it may have been stolen from the wreck of one of these vessels which occurred on the Blasquet Islands within view of the place were it was found.

Observations of Occultations of Stars by the Moon, and of Phenomena of Jupiter's Satellites, made at the Royal Observatory, Greenwich, in the Year 1873.

(*Communicated by the Astronomer Royal.*)

Occultations of Stars by the Moon.

Day of Obs. 1873.	Phenomenon.	Tele-scope.*	Moon's Limb.	Mean Solar Time of Observation. h m s	Observer.
May 1	Disapp. of 39 Geminorum	G. Eq.	Dark	8 22 6·8	C
"	" Arg. +26°. No. 1410	"	"	8 44 21·8	"
July 4	" λ Virginis	Altaz.	"	10 15 47·0	E
Aug. 9	" τ^2 Aquarii	G. Eq.	Bright	10 5 5·4	G
Oct. 3	" τ^2 Aquarii	Altaz.	Dark	7 47 31·5	E
Oct. 13	Reapp. of λ Cancri	E. Eq.	"	13 0 53·9	C
Dec. 1	Disapp. of *o* Arietis	"	"	12 42 54·4	"
Dec. 24	" τ^2 Aquarii	Altaz.	"	4 31 52·7	A D
"	Reapp. of τ^2 Aquarii	"	Bright	5 28 51·5	"
"	" τ^2 Aquarii	E. Eq.	"	5 28 49·8	E

Phenomena of Jupiter's Satellites.

Day of Obs. 1873.	Satellite.	Phenomenon.	Tele-scope.	Mean Solar Time of Observation. h m s	Mean Solar Time from N. A. h m s	Observer.
Jan. 10	I. (*a*)	Ecl. disapp.	G. Eq.	10 16 4·6	10 15 43·1	H C
"	II. (*b*)	"	"	10 20 39·8	10 20 3·1	"
"	IV. (*c*)	Ecl. reapp.	"	10 54 22·3	10 54 29·2	"
"	IV.	Occ. dis. first cont.	"	13 33 59·1	13 43	"
"	IV. (*d*)	" bisec.	"	13 40 28·0		
Jan. 26	I. (*e*)	Ecl. disapp.	"	8 31 11·0	8 30 45·8	W C
Mar. 8	II. (*f*)	" reapp.	"	10 2 28·7	10 2 53·8	L
Mar. 13	I.	" "	"	11 5 48·8	11 6 16·5	E
Mar. 14	III. (*g*)	" "	"	7 5 42·7	7 5 20·9	A D
"	I.	Tr. egr. first cont.	"	7 45 36·2	7 50	H C
"	I. (*h*)	" bisec.	"	7 47 35·9		
"	I.	" last cont.	"	7 49 20·6		
Mar. 22	I. (*i*)	Ecl. reapp.	"	7 29 8·6	7 29 22·0	A D
Mar. 26	IV. (*k*)	Tr. egr. bisec.	E. Eq.	10 1 20·2	10 7	C
"	IV.	" last cont.	"	10 5 19·6		

* The clear aperture of the object-glass of the Great Equatoreal is 12¾ inches. of the East Equatoreal 6·7 inches, and of the Altazimuth 3¾ inches.

Day of Obs. 1873.	Satellite	Phenomenon.	Tele-scope.	Mean Solar Time of Observation. h m s	Mean Solar Time from N.A. h m s	Observer.
Mar. 27	I. (*l*)	Ecl. disapp.	G. Eq.	14 55 7·5	14 55 17·8	A D
Mar. 28	III.	Occ. dis. first cont.	„	7 43 52·8		
„	III. (*m*)	„ bisec.	„	7 48 22·1	7 46	W C
„	III.	„ last cont.	„	7 52 11·4		
„	III. (*n*)	„ first cont.	E. Eq.	7 47 9·2	7 46	C
„	III.	„ last cont.	„	7 51 38·4		
„	I.	Tr. egr. first cont.	G. Eq.	11 20 46·7		
„	I.	„ bisec.	„	11 22 1·5	11 23	„
„	I.	„ last cont.	„	11 23 46·2		
„	III.	Occ. reapp. last cont.	„	11 25 15·9	11 28	„
„	III. (*o*)	Ecl. disapp.	„	11 34 34·4	11 32 1·6	„
Apr. 9	II. (*p*)	„ reapp.	„	9 47 9·9	9 47 20·8	H C
Apr. 21	I.	„ „	E. Eq.	9 37 6·6	9 37 4·9	C
Apr. 29	I.	Tr. egr. first cont.	G. Eq.	7 37 43·5		
„	I. (*q*)	„ bisec.	„	7 39 28·2	7 41	H C
„	I.	„ last cont.	„	7 41 57·8		
May 24	IV. (*r*)	Ecl. reapp.	„	10 48 20·4	10 48 10·4	L

Notes.

(*a*) The first sensible diminution of brightness was noticed $1^m\ 30^s$ before the time recorded above.

(*b*) A diminution of brightness was noticed $2^m\ 30^s$ before the recorded time.

(*c*) The observation was not very satisfactory, owing to cloud; but the observer considers that the time noted is not more than a few seconds late.

(*d*) The satellite had not totally disappeared three minutes after the time recorded at this observation, but *Jupiter* became so tremulous that it was impossible to estimate the time of last contact. Magnifying power used = 310.

(*e*) Bad definition. Power = 285.

(*f*) The satellite was at its full brilliancy about three minutes after the time recorded above. Power = 220.

(*g*) The satellite did not attain its full brightness till six or seven minutes after the recorded time. Power = 310.

(*h*) Observation good; the satellite was distinctly visible on the planet's disk at first contact. Power = 220.

(*i*) The satellite had attained its full brightness three minutes after the time recorded above; both the planet and satellite were very tremulous. Power = 500.

(*k*) Images very bad. Power = 130.

(*l*) The satellite was at its full brightness $2^m\ 37^s$ after the time recorded above; the images were not good, the sky being rather misty. Power = 220.

(*m*) Definition very good, though light passing clouds occasionally made the planet very faint. The observed time of last contact is considered fairly accurate. Power = 310.

(*n*) Satellite faint; sky very thick.

(*o*) The first diminution of light was noticed 2^m 35^s before the time recorded above.

(*p*) Observation good; the satellite attained its full brightness about three minutes after the time recorded above. Power = 310.

(*q*) Observation good; the satellite at first contact was distinctly seen on the disk of *Jupiter*. Power = 220.

(*r*) The satellite attained its full brilliancy 4^m 58^s after the time recorded above. Power = 60.

The initials W C, E, C, L, A D, H C, and G, are those of Messrs. Christie, Ellis, Criswick, Lynn, Downing, Carpenter, and Goldney.

Note on a Paper which appeared in the last Supplementary Number of the Monthly Notices (page 533). By Richd. A. Proctor, Esq.

I so far depart from a determination announced last summer, and sufficiently indicated in my last contributions to the *Monthly Notices* of the Royal Astronomical Society, as to send the present Note, the object of which is to express my regret for the circumstances which led to the appearance in the *Monthly Notices* of a paper "On the Antarctic and sub-Antarctic Regions suitable for observing the Transit of 1874." If I had known earlier of the arrangements described by Sir George B. Airy, at the November meeting of the Society, my paper would not have appeared, since in fact it could have served no useful purpose. Those arrangements seem to me to meet the requirements of the occasion as well as can, at the present time, be expected.

I take this opportunity of making three remarks relating to matters mentioned at the November meeting of the Society:—

First, I have never questioned in the slightest degree the energy with which the Astronomer Royal for England is superintending the details of the proposed expeditions.

Secondly, I have not urged as desirable the occupation of any specified stations in Antarctic or sub-Antarctic seas; my suggestions (the justice of which I remain convinced of) have related to the *search* for such stations in due time. These suggestions I regard as calculated, *when they were made*, to be extremely useful.

Thirdly, the above-mentioned essay in the *Monthly Notices* of the Society was accompanied by a foot-note clearly indicating that the paper was inserted on my responsibility alone.

Westminster Hotel, New York,
1873, *December* 26.

Note on the suspected Change of Position and Magnitude of the Stars between ε and ζ Lyræ. By E. Dunkin, Esq.

It was mentioned by Capt. Noble, at the January meeting, that Mr. Prince had been examining a drawing of this group made by the Rev. W. R. Dawes many years ago, and that he had discovered that it agreed generally with that which was given in the last number of the *Monthly Notices* as the appearance of the group at the present time. Mr. Lassell also explained to the Meeting that a drawing made by him many years ago represents the components of the group in the same relative position and magnitude as in that sketched by Mr. Prince. The evidence thus brought forward clearly proves that no sensible change has taken place either in the position or magnitude of the intermediate stars, and that the representation of the group given in the *Celestial Cycle* is inaccurate. Unfortunately, Admiral Smyth made no separate measurements of the three intermediate stars in question, and it is very probable that they were inserted in his diagram without much attention to their accurate position. Dr. De La Rue and others also remarked at the meeting, that a comparison with the diagrams in the *Celestial Cycle* should never be relied on as evidence of change, and it is therefore necessary, when comparing observations with those in the *Cycle*, to depend alone upon the measures given in the text.

Kenwyn, Kidbrooke Park Road, Blackheath,
1874, *January* 10.

Observed Position of ξ Ursæ Majoris in December 1873. By Wentworth Erck, Esq.

Epoch.	Position.	Mean.
	341°·15	
1873.936	1·39	341°·68
	2·50	

Each of the above is the mean of 10 measures:

Mean sidereal time of observation $=10^{h}\ 20^{m}$, when the components were nearly in the vertical:

As to the distance; clear space equal one-third diameter of apparent disk as observed with 7½-inch aperture:

The components were in contact with 6-inch aperture. Hence the distance was estimated at 0″·8.

As a comparison star I measured γ *Arietis*, said by Dawes to be unchanged since 1830. I found the position thereof to be 358°·28.

Telescope, the 7½-inch Alvan Clark: Power 750.

Sherrington, Bray,
1873, *December* 19.

Observations of Double Stars. By C. E. Burton, Esq.

No.	Name of Star.	Position. °	Distance. ″		Epoch.	No. of Measures.
1.	Castor	237·8	5·25		1873·981	15
2.	ζ Cancri	143·3	0·6	estimated	1873·956	10
3.	ξ Ursæ Majoris	342·4	0·8	,,	1873·945	10
4.	32 Orionis	189·8	0·5	,,	1873·967	5

1. The distance is the result of two measures on each side of the zero, differing by 0″·1. Observed on two nights with powers of 400 and 940.
2. Observed on two nights, five measures with power 400 and five with 940.
3. Power 400.
4. Measures extremely difficult; power 940. This star is evidently a binary.

The black spaces between the disks of the last three doubles were carefully estimated as follow:—

ζ Cancri,	with power	400, one-half apparent diameter of disk of either star.
,,	,,	940, barely equal to apparent diameter of disk of A.
ξ Ursæ Majoris	,,	400, apparent diameter of disk of either star.
32 Orionis	,,	940, apparent diameter of disk of B.

The observations were made with a 12-inch Newtonian equatoreal and position-wire micrometer.

Loughlinstown, Co. Dublin,
1874, *January* 7.

Two Occultations of τ^2 Aquarii observed during 1873. By Captain Noble.

The first of these occultations occurred on August 9, 1873, when the Moon was very low down.

At the time of disappearance, which took place at the bright limb, the bubbling and boiling were so tremendous that when the star was nearly in contact with the lunar limb, the latter seemed actually to involve and overlap it by its twists and twirls. Nevertheless, the actual disappearance was fairly observed at $19^h\ 18^m\ 32^s$ L. S. T. $= 10^h\ 4^m\ 41^s{\cdot}2$ L. M. T.

The re-appearance took place, of course, at the dark limb; but the Moon being only 1·8 day past the full, pretty near to the illuminated part of her western limb. It was quite sharp, and happened at $20^h\ 19^m\ 6^s{\cdot}5$ L. S. T. $= 11^h\ 5^m\ 5^s{\cdot}8$ L. M. T. The orange tint of the star was very perceptible on its emersion. Power 255, on my 4·2-inch Ross equatoreal, adjusted on a star.

The second occultation was that of December 24.

The star disappeared instantaneously at the Moon's dark limb at $22^h\ 45^m\ 24^s{\cdot}1$ L. S. T.$=4^h\ 32^m\ 19^s{\cdot}8$ L. M. T.

Its re-appearance was scarcely instantaneous; rather resembling the rising of a tiny planet from behind the bright limb of the Moon. It happened at $23^h\ 41^m\ 56^s$ L. S. T. $= 5^h\ 29^m\ 36^s{\cdot}5$ L. M. T. Power 255, adjusted on the star.

Forest Lodge, Maresfield, Sussex.

Observations of Faye's Comet (VI., 1873) *at the Observatory at Marseilles.*

This Comet appears to have been observed only at Marseilles by M. Stéphan. On each day of observation it was extremely faint. An observation made on September 3 is inserted on page 42, vol. xxxiv. of the *Monthly Notices*, and two others made on November 28 and 30 give the following places:—

1873.	Time of Observation, Marseilles M. T.			Observed R. A.			Observed N. P. D.			Comparison Star.
	h	m	s	h	m	s	°	′	″	
Nov. 28	16	24	55	9	16	19·75	89	37	25·6	*a*
Nov. 30	17	11	2	9	17	13·34	89	54	37·8	*b*

The corrections to the ephemeris in the *Berliner Jahrbuch* are:

	R. A.	N. P. D.
	s	″
Nov. 28	+0·73	−5·7
Nov. 30	−0·35	−4·0

Mean Positions of the Comparison Stars for 1873·0.

	Name of Star.	Mag.	R.A.			N.P.D.		
			h	m	s	°	′	″
a	W. B. (1) IX. 278	9	9	14	25·12	89	38	3·0
b	W. B. (1) IX. 307	8	9	15	40·72	89	53	11·8

The observation on November 28 was made under unsatisfactory circumstances. The comet was excessively faint, and only visible by glimpses. On November 30, the observation was a good one.

Note on the Position of the Small Stars near ε Lyræ.
By J. M. Wilson, Esq.

The following positions were obtained in July 1872:

	°
C D	142·0
A C	173
A B	17·1
A E	134·7
A F	180·5
A G	167·5
C E	36·6
C F	338·4
C G	1·2
E F	248·9
E G	268·5
F G	39·2

On laying these out, the diagram accompanying this Note is obtained. The position of G is not accurately obtained from them, as the lines do not truly intersect in one point. The greatest error possible in its position is 2″. The faint stars would not bear any illumination of the field, and the measures were taken with a parallel-wire micrometer.

Temple Observatory, Rugby,
1874, *January* 15.

The diagram forwarded by Mr. Wilson gives the relative positions of the stars very nearly the same as in that sketched by Mr. Prince, inserted on p. 86 in the December number of the *Monthly Notices.*—[Editor.]

Le Verrier's Tables of Jupiter.

In the *Comptes Rendus* for January 12, 1874, M. Le Verrier announces that he has completed his new Tables of *Jupiter* as a supplement to his researches on the theory of the motion of that planet.

"The theory on which the Tables are founded has been developed in four papers laid successively before the Academy at the meetings of May 20, August 26, November 11, 1872, and of March 17, 1873.

"The observations with which it has been compared are those made at Greenwich between 1750 and 1830, and between 1836 and 1869, and at Paris between 1837 and 1867.

"The theory is in complete accordance with the observations.

"Hence there exists no influence other than those already known to us, which appears to have any sensible effect on the motion of *Jupiter.* The influence of the minor planets is quite insensible."

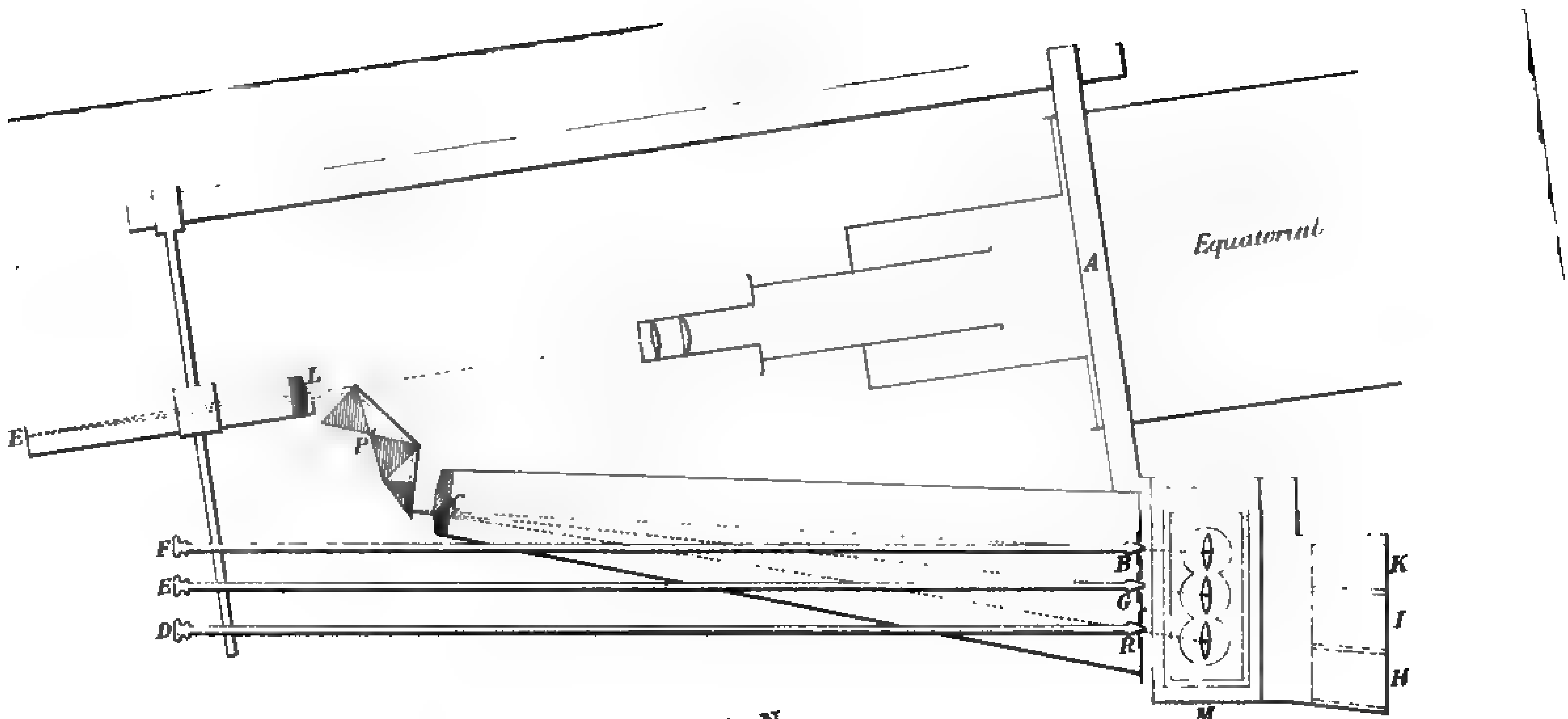
New Elements of Minor Planet (110) *Lydia.*

The following new elements of this planet have been calculated by Dr. H. Oppenheim:—

Epoch 1874, February 9·5, Berlin M. T.

	°	′	″	
L	136	18	48·7	
M	164	33	24·9	
π	331	45	23·8	Mean Equinox, 1870·0.
☊	57	8	23·7	Mean Equinox, 1870·0.
i	5	59	16·2	Mean Equinox, 1870·0.
ϕ	4	32	46·3	
μ			781·84272	
log. a			0·437925	

With these elements Dr. Oppenheim has computed ephemerides on two hypotheses for the use of observers who may be inclined to search for the planet with an equatoreal. The computed magnitude is 11·5.



PLAN
of a
NEW PHOTOMETER.

Spottiswoode & Co. lith. London

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

Vol. XXXIV. *February* 13, 1874. No. 4.

Professor Cayley, F.R.S., President, in the Chair.

George Creaser, Esq., Meltham, Huddersfield, and
Arthur Edward Donkin, Esq., Exeter College, Oxford,

were balloted for and duly elected Fellows of the Society.

Report of the Council to the Fifty-fourth Annual General Meeting of the Society.

Progress and present state of the Society:—

	Compounders	Annual Subscribers	Non-resident	Patroness, and Honorary	Total Fellows	Associates	Grand Total
December 31, 1872	193	321	9	2	525	48	573
Since elected ...	+8	+23	...	...	...	...	...
Deceased	−6	−6	−1	...	...	−2	...
Removals	+2	−2	...	...	...	...	...
Resigned, &c. ...	...	−11	...	...	...	...	...
December 31, 1873	197	325	8	2	532	46	578

Mr. Whitbread's Account as Treasurer of the Royal

RECEIPTS.

	£	s.	d.	£	s.	d.
Balance of last year's account				428	8	0
By Dividend on £3,600 Consols	53	2	0			
" " £5,200 New 3 per Cents. ...	76	14	0			
" " £3,600 Consols	53	6	6			
" " £5,200 New 3 per Cents. ...	77	0	6			
				260	3	0
Received for arrears of contributions	132	0	0			
" 186 Annual contributions	390	12	0			
" 24 Admission fees	50	8	0			
" First year's contributions ...	30	9	0			
				603	9	0
" 8 compositions				168	0	0
" Sale of publications at the Rooms of the Society	33	7	6			
By Messrs. Williams and Norgate	33	4	0			
				66	11	6
				£1,526	11	6

Astronomical Society, from Dec. 31, 1872, to Dec. 31, 1873.

EXPENDITURE.

	£	s.	d.	£	s.	d.
Salaries:—						
Editor of Monthly Notices	60	0	0			
Assistant Secretary	130	0	0			
Commission on Collecting	31	17	0			
				221	17	0
Taxes:—						
Land and Assessed	8	18	11			
Poor Rate	13	6	4			
Other Rates	5	17	6			
				28	2	9
Bills, &c.:—						
Strangeways, printer...	360	19	1			
Rev. C. Pritchard, Sir J. Herschel's Catalogue	30	0	0			
Wesley, engraver	85	0	0			
Whiteman "	18	10	0			
Pound "	38	2	6			
Malby	60	0	10			
Browning, instruments	14	5	3			
L. Wyon, medals	57	15	0			
Sun Fire Office Insurance	7	15	6			
				672	8	2
Miscellaneous:—						
House expenses	23	7	0			
Stamps and postages	62	4	10			
Books and parcels	4	7	6			
Expenses of evening meetings	13	13	0			
Waiters attending	3	17	0			
Coals and wood	12	0	0			
Gas	8	3	7			
Repairs	2	3	0			
Sundries	12	4	3			
Bankers' commission on cheques	0	1	2			
				142	1	4
Mrs. Jackson Gwilt's annuity				8	18	2
				1,073	7	5
Balance at Bankers'				453	4	1
				£1,526	11	6

Examined and found correct,

Henry Perigal, }
C. G. Prideaux, } *Auditors.*

Assets and Present Property of the Society, January 1, 1874:—

	£ s. d.	£ s. d.
Balance at Bankers'	......	453 4 1
3 Contributions of 9 years' standing... ...	56 14 0	
1 " 8 "	16 16 0	
9 " 7 "	132 6 0	
4 " 6 "	50 8 0	
4 " 5 "	42 0 0	
5 " 4 "	42 0 0	
14 " 3 "	88 4 0	
28 " 2 "	117 12 0	
38 " 1 "	79 16 0	
Balances of several Accounts	14 14 0	
		640 10 0
Due for Publications	......	1 9 0
£5,200 New 3 Per Cents (including Mrs. Jackson Gwilt's Gift, £300).		
£3,600 Consols, including the Lee Fund (£100) and Turnor Fund (£500).		
Unsold Publications of the Society.		
Various astronomical instruments, books, prints, &c.		
Balance of Turnor Fund (included in Treasurer's Account) ...		197 1 9

Stock of volumes of the *Memoirs*:—

Vol.	Total	Vol.	Total	Vol.	Total.
I. Part 1	8	XIV.	386	XXIX.	443
I. Part 2	48	XV.	170	XXX.	292
II. Part 1	66	XVI.	195	XXXI.	170
II. Part 2	30	XVII.	168	XXXII.	198
III. Part 1	82	XVIII.	173	XXXIII.	203
III. Part 2	101	XIX.	178	XXXIV.	188
IV. Part 1	99	XX.	176	XXXV.	156
IV. Part 2	101	XXI. Part 1	216	XXXVI. (with M. N.)	242
V.	121	XXI. Part 2	100	XXXVI. (without)	24
VI.	147	XXI. (together).	85	XXXVII. Part 1	307
VII.	171	XXII.	178	XXXVII. Part 2	321
VIII.	157	XXIII.	173	XXXVIII.	320
IX.	160	XXIV.	179	XXXIX. Part 1	385
X.	169	XXV.	193	XXXIX. Part 2	407
XI.	159	XXVI.	196		
XII.	186	XXVII.	453		
XIII.	195	XXVIII.	411		

The instruments belonging to the Society are as follow :—

The *Harrison* clock,
The *Owen* portable circle,
The *Beaufoy* circle,
The *Beaufoy* transit,
The *Herschelian* 7-foot telescope,
The *Greig* universal instrument,
The *Smeaton* equatoreal,
The *Cavendish* apparatus,
The 7-foot Gregorian telescope (late Mr. Shearman's),
The Variation transit (late Mr. Shearman's),
The Universal quadrant, by Abraham Sharp,
The *Fuller* theodolite,
The Standard scale,
The *Beaufoy* clock, No. 1,
The *Beaufoy* clock, No. 2,
The *Wollaston* telescope,
The *Lee* circle,
The *Sharpe* reflecting circle,
The *Brisbane* circle,
The *Baker* universal equatoreal,
The *Reade* transit,
A Sextant formerly belonging to Capt. Cook,
A Globe for showing the Precession of the *Equinoxes*.

The *Sheepshanks'* collection of instruments, viz.,—

1. 30-inch transit by Simms, with level and two iron stands.

2. 6-inch transit theodolite, with circles divided on silver; reading microscopes, both for altitude and azimuth; cross and siding levels; magnetic needle; plumbline; portable clamping foot and tripod stand.

3. $4\frac{6}{10}$ achromatic telescope, about 5 feet 6 inches focal length; finder, rack motion; two other micrometers; one terrestrial and ten astronomical eyepieces, applied by means of two adapters, with equatoreal stand and clock movement.

4. $3\frac{1}{4}$-inch achromatic telescope, with equatoreal stand; double-image micrometer; one terrestrial and three astronomical eyepieces.

5. $2\frac{3}{4}$-inch achromatic telescope, with stand; one terrestrial and three astronomical eyepieces.

6. $2\frac{3}{4}$ achromatic telescope, about 30 inches focus; one terrestrial and four astronomical eyepieces.

7. 2-foot navy telescope.

8. 45-inch transit instrument, with iron stand, and also Y's for fixing to stone piers; two axis levels.

9. Repeating theodolite, by Ertel, with folding tripod stand.

10. 8-inch pillar-sextant, divided on platinum, with counterpoise stand and horizon roof.

11. Portable zenith instrument, with detached micrometer and eyepiece.

12. 18-inch Borda's repeating circle, by Troughton.

13. 8-inch vertical repeating circle, with diagonal telescope by Troughton and Simms.

14. A set of surveying instruments, consisting of a 12-inch theodolite for horizontal angles only, with extra pair of parallel plates; tripod staff, in which the telescope tube is packed; repeating table; level collimator, with micrometer eyepiece; and Troughton's levelling staff.

15. Level collimator, plain diaphragm.

16. 10-inch reflecting circle, by Troughton, with counterpoise stand; artificial horizon, with metallic roof; two tripod stands, one with table for artificial horizon.

17. Hassler's reflecting circle, by Troughton, with counterpoise stand.

18. 6-inch reflecting circle, by Troughton, with two counterpoise stands, one with artificial horizon.

19. 5-inch reflecting circle, by Lenoir.

20. Reflecting circle, by Jecker, of Paris.

21. Box sextant and 3-inch plane artificial horizon.

22. Prismatic compass.

23. Mountain barometer.

24. Prismatic compass.

25. 5-inch compass.

26. Dipping needle.

27. Intensity needle.

28. Ditto ditto.

29. Box of magnetic apparatus.

30. Hassler's reflecting circle, with artificial horizon roof.

31. Box sextant and 2¼-inch glass plane artificial horizon.

32. Plane speculum artificial horizon and stand.

33. 2½-inch circular level horizon, by Dollond.

34. Artificial horizon roof and trough.

35. Set of drawing instruments, consisting of 6-inch circular protractor; common ditto; 2-foot plotting scale; two beam compasses and small T square.

36. A pentagraph.

37. A noddy.

38. A small Galilean telescope, with the object-lens of rock-crystal.

39. Six levels, various.

40. 18-inch celestial globe.

41. Varley stand for telescope.

42. Thermometer.

43. Telescope, with the object-glass of rock crystal.

To these must be added the following instruments, which had been employed in the Observations of the Total Solar Eclipse of 1870, and which, by a resolution of the joint Eclipse Committee of 1870, were transferred to the Royal Astronomical Society:—

Portable equatoreal stand,
Portable altazimuth tripod,
Four polarimeters,
Two Biquartz and Nicol's prisms,
Registering spectroscope, with prism,
Camera and chemicals, in box,
Two five-prism spectroscopes,
Cradle for telescope,
Eight-inch reflector and stand,
Spectroscope,
A small box, containing—
    Three square-headed Nicol's prisms,
    Two Babinet's compensators,
    Two double-image prisms,
    Three Savarts,
    One positive eye-piece, with Nicol's prism,
    One dark wedge.

These are now in the apartments of the Society, with the exception of the following, which are lent, during the pleasure of the Council, to the several parties under mentioned, viz.:—

The *Fuller* theodolite, to the Director of the Sydney Observatory.
The *Beaufoy* transit, to the Observatory, Kingston, Canada.

The *Sheepshanks* instrument, No. 2, to Mr. Huggins.
Ditto ditto No. 4, to Rev. C. Lowndes.
Ditto ditto No. 5, to Mr. Birt.
Ditto ditto No. 6, to the late Rev. J. Cape.
Ditto ditto No. 8, to Rev. C. Pritchard.
Ditto ditto No. 9, to the Director of the Sydney Observatory.
Ditto ditto No. 41, to Rev. C. Pritchard.
Ditto ditto No. 43, to Mr. Huggins.
The 6-inch circular protractor, to Mr. Birt.
Tripod stand. Mr. Chambers.

Lent on account of the Eclipse Expedition:—

One polarimeter. Mr. Ranyard.
One ditto. B. A. Eclipse Committee.
One Biquartz and Nicol's prism. Mr. Ranyard.
Camera and chemicals, in box. B. A. Eclipse Committee.
One five-prism spectroscope. B. A. Eclipse Committee.
One ditto Mr. Lockyer.
Cradle for telescope. Mr. W. A. Harris.

The Gold Medal.

The Council have awarded the Gold Medal of the Society to Professor Simon Newcomb, for his researches on the Orbits of *Neptune* and *Uranus* and other contributions to mathematical astronomy. The President will explain the grounds of this award in the usual manner, before the close of the meeting.

Printed Transactions of the Society.

Although no addition has been made to the published volumes of *Memoirs* since the last anniversary, yet two very important volumes are in progress, and they will probably appear during the ensuing year. Volume XL. will contain the valuable posthumous Catalogue of Double Stars collected from all available sources by Sir John Herschel, the manuscript of which was bequeathed by him to the Society. The Catalogue has been prepared for the press, including the calculation of the precession for each star, under the supervision of a committee consisting of Professor Pritchard and the Radcliffe Observer, both of whom have reported favourably on the value of the work.

Volume XLI. of the *Memoirs* is intended to be devoted solely to a discussion and classified arrangement of the observations of solar eclipses which have occurred from 1842 to 1871. The preparation of this volume has been undertaken, at the request of the Council, by the Astronomer Royal, who has been ably assisted in the details of the preparation for the press by Mr. Ranyard.

The last volume of the *Monthly Notices* is the largest that has ever appeared. It clearly exhibits the general activity of the Society during the past year, as well as the progress of astronomical science. Several of the papers are of more than usual interest, and more than the ordinary number of plates illustrate some of the more important. The valuable charts of the transits of *Venus* of 1874 and 1882, drawn by Mr. Proctor, may be specially referred to. These show the lines of equal acceleration and retardation of ingress and egress, and also the lines of equal duration for internal contact in 1874, and for external contact in 1882. An interesting series of drawings of *Mars* and *Jupiter*, by Mr. Knobel, is inserted in the June number. Some beautiful drawings of *Jupiter*, exhibited at one of the evening meetings by the Earl of Rosse, have since been chromo-lithographed at his expense, and the use of the stone has been kindly offered by him to the Society. The Council therefore hope to insert a copy of this valuable plate in the March number of the *Monthly Notices*.

The Society's New Rooms in Burlington House.

The Council had expected that the Society would have been in possession of their new rooms in Burlington House in time

for this anniversary meeting. Many unforeseen causes for delay, however, have occurred; but the Council have now been assured by Mr. Barry, the architect, that in April next the decorations and fittings of the rooms will be completely finished. As a question of prudence, as well as of convenience, it will be probably thought necessary to defer the migration of the Society from Somerset House till the conclusion of the present session. This will give time for the walls to become perfectly dry, especially in the basement, and thus the rooms will be made more habitable for the Assistant-Secretary. It may be as well to record that the entrance to the rooms is the first door on the left hand on entering the quadrangle from Piccadilly.

OBITUARY.

The Council have to deplore the loss by death of the following Fellows and Associates since the last Anniversary:—

Fellows:—Rev. Temple Chevallier, B.D.
F. H. Elliott, Esq.
Rev. George Fisher, F.R.S.
D. A. Freeman, Esq.
R. W. S. Lutwidge, Esq.
William Mann, Esq.
J. R. M'Clean, Esq., M.P., F.R.S.
Rev. Jacob Morton.
Frank Robertson, Esq., late Royal Madras Engineers.
Sir David Salomons, Bart., M.P.
J. Stanistreet, Esq.

Associates:—Dr. Giovan B. Donati.
Comm. M. F. Maury.

The Rev. TEMPLE CHEVALLIER, Canon of Durham, and lately Professor of Mathematics at the University in that city, died at the Vicarage, Harrow Weald, the residence of his son-in-law, the Rev. R. J. Knight, on November 4, 1873. He had just entered his eightieth year, having been born at Badingham, in Suffolk, on October 19, 1794. The family from which he was descended was of French origin, having left France for Jersey in consequence of the troubles of the sixteenth century, and proceeded thence to England. The first of the family who settled in England was one Rudolph Chevallier. It is recorded of him in one of Cranmer's letters that he assisted Tremellius, the friend of Calvin, in reading Hebrew Lectures at Cambridge; for which he had a grant to be a "free denisen, and to enjoy the advowson of a prebend in Canterbury." The grant was dated in August 1552. He is said to have been tutor to the Princess Elizabeth, afterwards Queen. It is certain that in her reign he was

appointed Professor of Hebrew at Cambridge, in the year 1569. For many generations the Chevallier family has been settled in Suffolk, and several of the ancestors of the deceased have been clergymen of the Church of England.

Temple Chevallier was educated at the Grammar Schools of Dedham and Bury St. Edmunds, and thence proceeded to Pembroke College, Cambridge. In his Freshman's year he obtained the Bell Scholarship. He graduated in 1817, when he was placed second in the list of Wranglers, and gained the Second Smith's Prize. In 1819 he was elected to a Fellowship in his College, and afterwards became Fellow and Tutor of Catharine Hall. He was ordained in 1820, and was appointed shortly after to the Vicarage of St. Andrew the Great, Cambridge, which he held till he removed to Durham in 1834. During this period he filled the office of Hulsean Lecturer in 1826 and 1827. The subjects of his published lectures were "The Historical Types of the Old Testament," and "The Proofs of Divine Wisdom and Goodness to be derived from the Study of Astronomy." The latter of these shows him to have been a profound mathematical scholar, and that his mastery of the theory of celestial dynamics was most thorough and complete. In connection with this subject it should be mentioned, as has been suggested by a friend, that he never gained proper credit for the originality manifested in the manner in which he handled the subject, and that this course of lectures forms "the step in the argument between Chalmers' 'Astronomical Discourses,' and Whewell's 'Bridgewater Treatise.'"

While resident in Cambridge he published his translation of the "Epistles of Clement, Polycarp, and Ignatius," and of the "Apologies of Justin Martyr and Tertullian," a book which has achieved and maintained a standard position.

In 1834 he removed to Durham, as Professor of Mathematics, and Reader in Astronomy and Hebrew, in the recently-founded University, to the usefulness of which he very largely contributed. One feature of his work here should not pass unnoticed. He kept up the study of Newton at Durham, after it had been disused at Cambridge, Dublin, and Oxford; and he alone of all modern teachers carried his men not only through Sections 1–3 and 11, of Book I., but into Books II. and III., which was done in the now abandoned examination for the degree of M.A.

Besides his professorial work, it was mainly through his exertions that the Durham Observatory was founded, over which he presided as Director for thirty years. Although engaged in tutorial duties he has repeatedly shown his aptitude for original inquiry. He was the first to institute in England the regular, continuous observation of the Solar spots, which has since led to important results. The methods he employed in these observations were afterwards adopted by Mr. Carrington (at one time Observer at Durham), who has made a similar series of observations with marked success; and astronomers may perhaps feel disposed to regret that Mr. Chevallier's talents were too

much occupied by clerical and professorial work to admit of the full development of his powers in the field of original research. When he was obliged, through the infirmities of age, to resign his Professorship, and to relinquish his connection with the Observatory, he still continued most solicitous of its welfare, and to him is due all the credit of the maintenance of an efficient institution for the advancement of astronomical science in Durham. He was elected a Fellow of this Society on December 13, 1839.

The clerical duties, to which reference is made above, were the diligent supervision of the country parish of Esh, six miles from Durham, of which he was for thirty-five years the active and much-loved pastor, and which cure he only resigned in 1869. In 1865 he was appointed to a stall in the Cathedral by the present Bishop of Durham, Dr. Baring.

In addition to his mathematical acquirements, Professor Chevallier was a distinguished classical scholar, and largely acquainted with English literature. He was also thoroughly conversant with the French and Italian languages. He is the only Cambridge man who was Examiner both for the Mathematical and Classical Triposes in the same year (1826), and few men have covered so large an area of literary and scientific knowledge.

This notice must not close without recording the still higher qualities which Canon Chevallier displayed as a sincere Christian. He embraced with childlike simplicity the Scripture Revelation. He was imbued with a deeply reverent spirit, and his life was one of consistent piety and large Christian benevolence. He has been the liberal friend of very many. Many a poor student at Durham has he taken by the hand and helped through his College course. His genial, kindly character made him a favourite with all who knew him. Of singular modesty, high intellectual capacity, sincere earnestness in work, deep learning, and the greatest willingness to impart knowledge, he has left behind him a reputation that could hardly be increased within his sphere of action, while his numberless deeds of kindness and many social virtues render his memory still more beloved than admired. R. J. K.

F. H. Elliott, M.A., was born in London on May 27, 1819, and was a son of the well-known mathematical instrument maker, Mr. W. Elliott, in Holborn. After having received a private education he entered Christ's College, Cambridge, in July 1841, where he remained three years, and obtained his B.A. degree in January 1845. Afterwards he practised as a surveying engineer during the period of the Railway mania, in which capacity he acquired a practical knowledge of instruments, which in after-life he turned to excellent account. Subsequently he entered his father's business, which was removed to the Strand, and which by his activity and good business qualities was greatly increased.

He died suddenly, of apoplexy, in January 1873, and a striking

proof of the estimation in which he was held, not only by his friends but by those in his employ, was given by the great number who crowded to pay the last tribute of respect at his grave.

The Rev. GEORGE FISHER was born at Sunbury, on the 31st of July, 1794. One of a numerous family, and left from childhood to the guardianship of a widowed mother, he received but little early education, and, at the age of fourteen, when most boys of his position in life are enjoying advantages he would have appreciated to the utmost, employment was found him as a clerk in the Westminster Insurance Office. His strong mathematical and scientific tastes had already declared themselves, and such occupation as his must necessarily have been distasteful to him; yet when after some years he left it for work more in accordance with his inclinations, his employers gave substantial testimony to the respect which his diligence and devotion to duty had earned from them.

The ardent desire for knowledge, which never in extreme old age deserted him, now brought him into contact with men of distinguished rank in the scientific world to whom he would otherwise have remained unknown, and the names of Sir Joseph Banks, Sir Humphry Davy, Sir Everard Home, with many others, were always gratefully mentioned by him as having noticed with kindly sympathy and generously fostered his early love of science.

In 1817 he entered St. Catharine's College, Cambridge, with no greater store of information than he had contrived to gain during the hardly-won intervals of leisure which his London life afforded him. It is much to be regretted that Mr. Fisher's University career was interrupted, not only by the Polar voyage which he undertook in 1818, but by a serious illness, which, attacking him at the very time of examination, prevented his attaining all the success which his talent and industry deserved.

In the year 1818, by the recommendation of the President and Council of the Royal Society, Mr. Fisher was appointed Astronomer to an expedition then setting out in His Majesty's ships *Dorothea* and *Trent* (about to proceed to the Arctic regions), to make observations concerning the length of a pendulum vibrating seconds, for the determination of the figure of the earth, and to make other scientific observations, for which instruments were provided. Unfortunately, the vessels encountered a violent gale of wind and were disabled, returning to England in the latter part of the same year. However, the time was not entirely thrown away: experiments were made on the length of the pendulum at Spitzbergen, from which Mr. Fisher deduced the ellipticity of the meridian to be $\frac{1}{313}$, in latitude 79° 40′ N., which agreed sensibly with Sabine's previous determinations in latitudes 60° 10′ N. and 70° 10′ N. He was afterwards, on the same recommendation,

appointed Astronomer of the expedition commanded by Captain Parry, in the years 1821–2–3, to discover a North-west Passage; and being then in Holy Orders, he was, on the recommendation of the Bishop of London, directed by the Lords of the Admiralty to act as chaplain also, and formally entered the naval service. A large number of astronomical instruments and a portable observatory were embarked on board the *Fury*, and afterwards set up at Igloolik and Winter Island; and were used to such good purpose, that Captain Parry concludes the introduction to the second voyage in these words: "I have the most sincere pleasure in offering my testimony to the unabated zeal and perseverance with which, under circumstances of no ordinary difficulty from climate, and in spite of frequent ill-health, he (Mr. Fisher) continued to pursue every object which could tend to the improvement of astronomy and navigation, and to the interests of science in general."

The results of this expedition were arranged by Mr. Fisher, and published in 1825, and consist of an account of the chronometers, their rating by lunar observations while at sea, and by meridional transits of the Sun and other observations when on shore, and a great number of observations, taken principally by the officers accompanying the expedition, for the determination of geographical position, the dip and variation of the magnetic needle, and the changes in the tides. An important paper by Mr. Fisher, on Atmospheric Refraction, appears in the same volume, the results being deduced from nearly four thousand observations, most of them taken at temperatures as low as $-30°$ to $-50°$ Fahrenheit. The principal instrument used was a Repeating Circle, by Troughton, fixed on a cask filled with pebbles and sand, cemented firmly together and to the ground by pouring water upon it, which immediately froze, rendering the whole one solid mass, which could not be affected by any force that could be applied. The principal difficulties to be encountered in the use of this instrument, as found by previous Arctic explorers, were, first, that of moving the circle in azimuth, arising from the unequal contraction of the centre work; and secondly, that of the great contraction of the spirit within the principal level, this contraction causing the bubble to vary in length from 36 divisions at a temperature of $+30°$ to 105 divisions at a temperature of $-30°$ Fahrenheit. The second difficulty was partially obviated by the introduction of more spirit, and the stiffness of the circle was found afterwards to be caused principally by the frozen vapour in the joints, and was easily expelled by taking the instrument into the computing-room of the observatory, which was always kept temperate by a stove. The method employed to determine the refraction was by observing the difference between the Polar distances of high and low stars as they crossed the meridian, keeping the horizontal wire of the back telescope in line with an illuminated horizontal row of holes in the meridian-mark, in order to ensure perfect stability of the telescope. This method failed,

first, from the uncertainty of a distant terrestrial object being refracted alike during the interval of the meridional passage of the two stars; and secondly, the principle of repetition was lost. The method subsequently employed was simply comparing the observed meridional zenith distances of the Sun and low stars with the zenith distances computed from the Polar distance and the latitude of the place, Igloolik, on the N.E. coast of America, in latitude 69° 21′ 0″·62 N. Captain Parry and his officers also adopted the same plan at the ships (about half a mile distant) with their sextants; many trials, however, showed that the refraction for altitudes that could conveniently be taken with an artificial horizon so nearly agreed with the tabular refraction, that the difference was within the limits of accuracy attainable by the sextant at low temperature.

The method adopted the second winter at Winter Island was by measuring the distance between two stars nearly in the same vertical circle, one of them being a low star, and the other as near the zenith as possible. The true distance being computed, it is clear that the discordance is due to the difference of refraction of the two stars, that of the higher star being assumed to be correct. The greatest care being taken in these observations, the results show that, at a temperature of − 20° F., at an altitude of 4½°, the tables of Dr. Young were not more than ten or eleven seconds in defect; but when the temperature is as low as −41° the errors of the tables rapidly increase. Amongst the other observations made by Mr. Fisher we find a determination of the velocity of sound, giving mean results of 985·9 feet per second at −41° Fahrenheit, varying to 1069·9 feet per second at + 33°.

A vessel of air having been brought from Igloolik and submitted to Faraday for analysis, was found to contain only 20·58 per cent. of oxygen, a quantity which, according to Dr. Angus Smith's experiments in the Cornish mines, would be at least impure, an atmosphere which contains less than 20·6 per cent. of oxygen being marked *exceedingly bad.* At the suggestion of Mr. Fisher vessels containing various gases were sent out, some condensed by pressure. In many of the vessels the gases were reduced to fluids at low temperatures, and small crystals were found in the upper parts of the vessels, while in some only a few white deposits on the sides of the glass were seen, and others were not affected by the greatest artificial cold in which they were placed. This discovery of the liquefaction of gases, especially chlorine, took place in 1822, one year before the discovery was made by Faraday. Observations were also made on the variation, dip, &c., from which the magnetic force was observed to be "greater during the summer than the winter," a discovery attributed by Sir E. Sabine, in 1845, to the result of the Toronto observations.

After the return of the *Fury* and *Hecla* to England in November 1823, Mr. Fisher was employed for about a year in preparing and passing through the press his valuable observations and

papers. He was elected a Fellow of the Royal Society in 1825, and in 1827 a Fellow of this Society; and was a Member of the Council from 1835 to 1863, several times filling the office of Vice-President. From the year 1828 to 1832 he was employed as Chaplain to H.M.S. *Spartiate* and *Asia*, continuing his magnetic observations at London, Ryde, Malta, and various ports on the coast of the Mediterranean, and on his return retired from the Navy on half-pay. The following year he presented his observations in a paper to the Royal Society on the "Relative forces soliciting a magnetic needle"; on the "Variation in the intensity of those forces"; and on the "Diurnal oscillations in the direction of a magnetic needle," the needle being suspended horizontally.

In the year 1834 Mr. Fisher was offered by Lord Auckland (then First Lord of the Admiralty), the choice of the Greenwich Hospital living of Falstone, in Northumberland, or the Chaplaincy and Head-Mastership of the Greenwich Hospital School, with an intimation that "he would be better pleased by the acceptance of the latter, as it was more in accordance with his former pursuits." The latter office was accepted, and Mr. Fisher entered on his duties at a time when the school was rising from a very low state, morally, physically, and intellectually, and by his indomitable perseverance in carrying out schemes for the good of the establishment, his calm demeanour, his tact, and even-handed justice dealt to all with whom he came in contact, combined with the respect and affection he succeeded in gaining from his colleagues, the School became, as described by the late Professor Moseley, "second to no other similar school in Europe." During his time of office the Royal Naval School at Greenwich assumed a highly practical character as a "hot-bed of navigation," supplying to all the Navigation Schools on the coast masters who instructed officers of the Mercantile Marine in navigation, &c., in that "peculiarly efficient mode which characterises this school," as well as several Naval Instructors and Navigating Officers of Her Majesty's Fleet. To carry out the instruction of the Admiralty of making the school a "nursery of skilful navigators," an observatory was planned by, and erected under, the superintendence of Mr. Fisher, and this continued under his supervision for thirteen years, to the great advantage of those who received his practical instruction.

In the year 1845 an application was made to him by the late Lord Herbert to undertake the writing of elementary text-books for the use of the school. Two elementary works were written by Mr. Fisher in consequence, the first on Algebra, and the second on Geometry, in which the Elements of Euclid were departed from, and an attempt made to introduce modern methods. He was appointed Principal of the School in 1860, and finally retired in 1863.

During these later years he wrote principally on the nature and origin of the Aurora Borealis in Arctic regions, advancing

the theory that the Aurora is "an electrical phenomenon arising from the positive electricity of the atmosphere developed by the rapid condensation of the vapours in the act of freezing;" and a most valuable paper was contributed by him to the seventh edition of Riddle's *Navigation* on "Circular Arc Sailing," a highly practical and instructive attempt to modify great Circle Sailing when the latitude into which the ship would be led is so high as to render navigation dangerous. His great age and increasing infirmity prevented Mr. Fisher during the last few years of his life from taking part in the meetings of the Society; yet till the very day before his death he never failed to take a lively interest in its transactions, and the progress of Astronomical Science had an increasing source of delight to him.

Always of a singularly childlike and contented disposition, the companionship of those dearest to him, and of his books, were all he needed for happiness; and after eleven years' tranquil enjoyment of well-earned rest, he passed gently and painlessly away on the 14th of May, 1873, loved by all who knew him throughout the course of his blameless life of nearly eighty years. A. F.

William Mann, late First Assistant at the Royal Observatory, Cape of Good Hope, and attached to the staff of that Observatory during a period of thirty-two years, was the third son of Major-General Cornelius Mann, of the Royal Engineers. He was born at Lewisham, in Kent, October 25, 1817, and died at Claremont, near Cape Town, on the 30th of April, 1873, at the age of fifty-five years. He was elected a Fellow of this Society on the 10th of March, 1871.

The subject of this memoir was under the care of private tutors in England until 1830, when he was in the thirteenth year of his age. At that time his father received the appointment of Commanding Royal Engineer at Gibraltar, and he went with his family to that station, and continued his studies there, in the hope of entering in due time the Royal Military Academy, at Woolwich. The regulations, however, prevented this, in consequence of his having an elder brother already in the Academy. He accordingly had to turn his attention to some other pursuit; and through the kind recommendation of Admiral Shirreff was ultimately appointed Second Assistant at the Royal Observatory, at the Cape. He visited England to complete his preparations for the appointment in 1837, and joined the staff of the Royal Observatory in October 1839, being then twenty-two years old.

Mr. Mann took with him to the Cape various instruments and forms of apparatus that had been prepared for re-measuring La Caille's Arc of the Meridian. During the first six years of his service he was chiefly employed in operations connected with this work, under Mr. (now Sir Thomas) Maclear; and it was while so employed that his sterling value as a votary of science became apparent. He had great fondness for the higher branches

of the mathematics, and great facility in applying their doctrines and methods, and with this natural aptitude he possessed also a remarkable skill in mechanical manipulations. In addition to these natural gifts he was persevering and patient to the last degree, and fastidiously scrupulous in the performance of all tasks of duty. While working upon the verification and extension of La Caille's Arc he was often exposed for months at a time in the wild country of the Clanwilliam District, lying between 200 and 300 miles north of the Cape, occasionally being three months without other shelter at night than the open sky. The kind of life he had to lead at this time is very graphically told in a brief extract from one of his letters, in which he describes how he established the station some seventy miles beyond the Sneeuw Kop peak of the Cedar Mountains. A mountain had been seen over the broken Karroo country from the Sneeuw Kop, in a north-eastern direction, which seemed likely to answer for a connected station; but the intervening tract of land was so difficult, and the means of transport so utterly insufficient, that it was out of all question to attempt to send a party of men to the place to establish the signal. Mr. Mann, therefore, volunteered to make his way to the mountain, and to conduct the signals by himself. He accordingly started, with only one Hottentot attendant, a bag of rusks, and a map of the country. After three days' wandering through the wildest and most desolate territory, entirely destitute of water, he came providentially upon a small stream, late one evening, and slept on a bare rock near the base of the desired mountain. His narrative of his further proceedings is then continued in the following words: "Next morning I called a council of war, consisting of myself, to debate on what was to be done. I found there was bamboo enough to last the horses two days, and we two mortals might possibly exist that period upon the quantity of bread which remained; and as the time was precious in our operations, and if I left our present position I did not know how long we might be wandering about before we came to a house, I determined to make this my head-quarters. So leaving my servant in charge of the horses, I prepared to ascend the mountain for the purpose of making the necessary signals. I took with me two rusks, all that could be spared, and which were to last me as many days, and as much water as my pocket-pistol could hold, which was about one draught. I had about 2,000 feet of mountain to climb, and a heavy load to carry; and taking into account the heat of the weather, and the want of a breakfast, I was *rather* tired when I at last arrived at the top. I soon established my signal, and sat all the rest of the day reading it. I was obliged to fill my mouth with pebbles to keep it moist. Night came on, and, hungry and thirsty, I laid myself down to sleep. There was not a stone or a bush on the top to afford the least shelter; it blew a gale of wind, and the night was as cold as the day had been hot. Next morning at sunrise I was again signalling away, and

about noon to my great joy I saw the signal from the Sneeuw Kop station for me to leave! I was not long in packing up my traps and getting down the mountain. I found my servant and the horses looking very miserable, but cheered the former with the news that I had discovered with my telescope a farm-house a few hours off." He then goes on to tell how he made for the house, but alas! only to find it a ruined and deserted homestead. He finally reached the Sneeuw Kop Station, after three other days of painful travel. This little incident is worthy of extract, as illustrating in a forcible way what measuring an arc of meridian in the wilds of South Africa means. It is not surprising that even a naturally vigorous and good constitution felt the strain of such work and exposure as this. In the year 1846 Mr. Mann had to visit England for the restoration of his impaired health; and Mr. Maclear wrote upon that occasion: "I feel the loss of Mr. Mann's services especially at the present juncture. His powerful intellect, his unflinching integrity and industry, enable me to trust him with confidence on all occasions and in every department, whether at the Observatory or on the Triangulation, being certain that whatever is practicable he will accomplish, and that what he does will be sure to be well done."

Mr. Mann returned to the Cape in improved health in 1847, and for the next five years was actively and incessantly engaged in the current work, both of observing and calculating, in the Observatory. In 1852 he was commissioned to proceed to England, to make himself acquainted with the mechanism of the large Transit Circle then constructing for the Cape. He returned to the Cape with this instrument in his charge, and then proceeded to erect it at the Observatory, a work of hardly conceivable difficulty under the circumstances, as at that time there was absolutely no skilled labour, of the class required, at the Cape. If Mr. Mann had not been unusually skilful himself in the use of his hands and in the delicate application of tools, as well as familiar with every essential point in the mounting of the complicated instrument, the work would have had to stand over until artisans could be sent from England at very large cost. As it was, the noble Transit Circle, which is very nearly a duplicate of that in use at Greenwich, was mounted in a most complete and efficient manner. The writer of these lines remembers an interesting and notable illustration of the accuracy and skill with which this difficult task was performed, which he had from Mr. Mann himself in familiar conversation. In one part of the operations the exactness of the position had to be determined by the image of a central wire being reflected from a still surface of mercury. Mr. Mann, having had the pivots of the horizontal axis placed rudely in position on the piers, was aghast to find that he had no image of the wire in the field of view. After some puzzle and some delay, to his inexpressible delight he found on tapping the tube that the reflected image was there, but concealed behind the wire. The work had been so perfectly planned

and executed, and the instrument was so absolutely in place, that the wire intercepted its own reflected image. Mr. Mann had no other assistance in the erection of this large instrument than such as he could get from Hottentot workmen trained by himself.

About the year 1853 Mr. Mann married Caroline, the second daughter of Mr. Maclear, Her Majesty's Astronomer at the Cape, and so, as a member of the family of his chief, became more closely interested in, and more intimately identified with, the Cape establishment.

At the latter part of the year 1859 Mr. Maclear paid a visit to England, and during his nine months of absence, the entire administration of the affairs of the Royal Observatory at the Cape was placed in Mr. Mann's hands. During that period he undertook the calculations that still remained necessary for completing the verification and extension of La Caille's arc of the meridian. These calculations were ultimately carried through by Mr. Mann himself. About this time he also entered very assiduously into comet-work, which always had great attraction for him; and it was during the long exposures to cold winter nights incident upon this, and especially while observing Encke's Comet, that he incurred the first attack of a disorder of the chest, under which he has since fallen. In 1866 he made a short visit to Natal, on account of the severe suffering that asthmatic disorder had brought upon him, and in 1867 extended his trip to England for a further measure of rest and change. After six months' recruiting he returned to his work, considerably refreshed, and then entered upon the reduction of the Cape observations for the preceding thirty-four years. He also continued his comet-work, and conceived an organised plan for the observation, reduction, and cataloguing of Southern stars, which unfortunately he was never able to carry through. Up to the year 1870, although unmistakeably suffering from impaired powers, he was unintermitting in his application to the work of the Observatory. At this time he had to meet the additional depression and vexation incident to the retirement of Sir Thomas Maclear from his position as Astronomer Royal at the Cape. Not long subsequently to this an outbreak of scarlet fever occurred in the neighbourhood of the Cape, and visited the Royal Observatory. Two of Mr. Mann's children died of the disease, and he himself suffered so severely that his life was for some time in jeopardy. Under the great care of his family and friends he survived the actual attack, but was so broken down by it that he found it impossible to resume his work, and resigned his appointment, retiring with a small pension for his past services. At the end of the year 1872 he was further reduced by bronchial disease, and from that time, with occasional fluctuations, gradually declined, until the last day of April in the following year, when he sank quietly to his rest, after a useful life of hard labour, leaving a widow and a large family of children to mourn his loss. Shortly before his death Her Majesty had

been pleased to grant him a small pension from the Civil List in recognition of his good service, but he was himself never aware of this gracious act. Mr. Gladstone very considerately ordered the payment of a sum equal to three years of this pension to Mrs. Mann.

Apart from the pursuits involved in his professional work and in his high mathematical and mechanical attainments, Mr. Mann had great fondness for archæological studies. He had a considerable knowledge of old coins, which he had studied in early life with his father. He had also some skill as an artist, and sketched from nature. The qualities, however, which will be most closely associated with his memory by his acquaintances and friends are the singular combination of gentleness and strength, of modesty and power, which were present in all he did. There never was a man who moved through a thorny path in life, in which the growing needs of a large family had to be provided for out of too limited and inelastic means, with more patient and resolute courage. It was only the few who had the privilege of his intimate acquaintance who were aware of the remarkable intellectual vigour and strength, the clear and exact thought, and the large sympathies and attainments that lay behind the sweet ever-present smile, the gentle word, and the all but absolute forgetfulness of self, which at all times made up his personal presence, and gave as it were the key-note of his character. William Mann, of the Cape Observatory, had not the pushing, self-asserting qualities which most win the admiration of the world, and which most certainly command its substantial rewards. But he had the capacity and powers which would have given him a much more distinguished position than he attained if he had been more favoured by circumstances. As it is, he has the reward of a full recognition of honest work, ably performed through a long self-denying life, and the sorrowing memories of a small circle of appreciative friends, some among whom delight also in the consciousness that simple, unaffected, and unquestioning piety went hand-in-hand in his daily life with his remarkable intellectual aspiration and mental power. R. J. M.

JOHN ROBINSON M'CLEAN was born at Belfast, in 1813. After the completion of his general education he applied himself assiduously to the task of preparing for the profession which he had chosen, that of a civil engineer. With this view he proceeded to the University of Glasgow, and after studying there with distinction he entered the office of Messrs. Walker and Burgess, where he continued for seven years.

In the year 1844 Mr. M'Clean began to practise independently, and from that time rapidly rose to eminence in his profession. As an engineer he was distinguished for practical skill and sound judgment, and his name is associated with numerous important works designed and carried to a successful issue by him, and many of which owed their origin to his wise foresight. The now important

and prosperous district of Barrow-in-Furness is indebted, in a great degree, for its marvellous recent development to his early appreciation of its capabilities, as well as to his engineering skill; its harbour, docks, and railways were all constructed under his direction. The South Staffordshire Railway, the Wolverhampton, Birmingham, and Dudley Railway, the South Staffordshire Waterworks, and many other large undertakings, were carried out under his superintendence. He was also, on the death of Mr. James Walker, appointed by Government to be engineer of the harbours of Dover, Alderney, and St. Katherine's, Jersey; and of the Plymouth Breakwater and Shovel Rock Fort.

The confidence reposed in Mr. M'Clean's talents and judgment was also further evinced by the numerous Royal Commissions on which he was called to act, including those on the designs for the Thames Embankment, the Commission on Railways, the Sanitary Commission, and others.

Mr. M'Clean was consulted by the late Emperor Napoleon, with reference to some of his contemplated improvements at Paris, and he carried out extensive works there for the Emperor. He was also one of the engineers invited by the Viceroy of Egypt to examine and report upon the Suez Canal. He was a member of the Institution of Civil Engineers, and President during the years 1864 and 1865, and was elected a Fellow of this Society on January 8, 1858. He was also a Fellow of the Royal and of other learned Societies, and represented the Eastern Division of Staffordshire in the late Parliament.

During the last years of his life Mr. M'Clean's health was seriously impaired, and he suffered from an illness of which he ultimately died on July 13, 1873. His private worth and active kindliness of heart, no less than his professional eminence, will cause him to live long in the memories of the wide circle of friends whom his genial disposition had attached to himself; and his numerous acts of unostentatious kindness and generosity will make his loss deeply regretted by those, and they are many, who have been aided and befriended by him in their struggles and difficulties. G. P. B.

Frank Robertson, late First Lieutenant Royal Madras Engineers, was born in London in the year 1838. He was the youngest and last surviving son of the late Robert Robertson, Esq., of Auchleeks, Perthshire. In 1854 he commenced his professional studies at Addiscombe, where he soon became one of the most successful students of the College, especially in chemistry, the prize for which he gained in his first term, an honour which till then had been considered as belonging exclusively to the senior students. After practically studying his profession at Chatham, he went in 1859 to Madras, having been appointed to the Madras Engineers, where he was employed on many important works; but in consequence of a severe illness, produced by an incautious exposure to the sun at Nagpore during the performance of his

duties, he returned to England in the winter of 1861 in a very weak condition. He had already superintended the construction of several military buildings to the satisfaction of his commanding officer, and doubtless would have further distinguished himself in his corps, but owing to some difficulty in his obtaining a medical certificate without considerable delay, which his dangerous state of health would not permit, he resigned his commission before leaving India.

Lieutenant Robertson had a great natural taste for civil engineering, both in theory and practice, and on his recovery he returned to India as a civil engineer, but his health again soon broke down, and he was once more driven home. His devotion to his profession was, however, so great that a third time he proceeded to India, when he soon found employment under the Department of Public Works, latterly at Murree, under the direction of Colonel Maclagan. In this capacity his talents were duly appreciated by the Government; but his health, which had never been perfectly restored since his first illness, compelled him, after a time, to relinquish his office, and to seek a more bracing climate. During the Franco-Prussian war he went to Darmstadt, where he joined a company of young men who devoted themselves to the care of the wounded soldiers. His health being again fairly restored, Mr. Robertson left England for India in January 1873, determining once more to brave the hot climate which, on so many previous occasions, had ever proved his greatest enemy. He was employed in the Punjaub on some important public works; among them a bridge of boats was constructed and maintained by him at a very critical time, when the railway bridge over the river Sutlej had given way, and when the other bridges of boats in the same locality had all been carried away by the floods. An extract from a characteristic letter written by Mr. Robertson to his sister, dated Phillour, June 4, 1873, exhibits his devotion to his work at a time when the strain on his mind was very severe: "I am so glad about the bridge. The Punjaub Government say it is the most important work in the whole division, and no doubt it is so; they have done their utmost to urge me to hold on till the railway bridge could be restored, and now it is all finished and all right. The other bridges are for mere road communication over the Jhelum, Ravi-chenab, Beas, &c., and all have given way weeks ago; mine alone bears the railway traffic, and each day it has been kept open the Government has gained 200*l.* sterling." A few months afterwards he suffered from an acute attack of dysentery, and was advised to return to Europe immediately. He left Bombay about the middle of September, and died on board the Peninsular and Oriental steamship *Tanjore*, near Aden, on September 27, 1873, at the early age of thirty-five.

In 1871 Mr. Robertson published a most valuable series of tables for facilitating the construction of arches, which cannot fail to be of great practical use to engineers. He was much

attached to this Society, of which he was elected a Fellow on February 11, 1859. E. D.

Sir DAVID SALOMONS, Bart., was born in the year 1797. Following the example of his father, a London merchant and underwriter, he entered at an early age into the busy affairs of commercial life, and soon became a most prosperous City merchant. Although, at this time, his thoughts were necessarily engrossed with the cares of business, giving him but little leisure-time for other occupations, yet he had a considerable taste for science, and especially for astronomy, which made him seek the society of men with kindred spirits, among whom were some distinguished Fellows of the Royal Astronomical Society. He was himself elected a Fellow of the Society on March 12, 1830, and he at once took a considerable interest in its proceedings at the ordinary meetings. For many years he continued in friendly association with the leading members of the Society, including our late esteemed President, Francis Baily; but though he was always ready with his counsel, the absorbing occupation of his time in his daily business prevented him from devoting that attention to astronomical pursuits which his natural taste for the science would otherwise have led him to follow. To enable him to carry out his magisterial duties in a satisfactory manner, as an Alderman of London, to which office he was elected in 1847, he studied law in middle life, and was called to the Bar at the Middle Temple in 1849. He served the office of Sheriff of London and Middlesex in 1835–36, being the first member of the Jewish persuasion elected to that important office; he was also High Sheriff of Kent in 1840, and Lord Mayor in 1855. He continuously represented the borough of Greenwich in Parliament from 1859 to the time of his death, which took place on July 18, 1873, at the age of seventy-six. It is a curious coincidence that Greenwich, the natural home of astronomy, was represented in Parliament from 1865 to 1868 by two Fellows of the Royal Astronomical Society, Alderman Salomons and Sir Charles Bright. Sir David was created a Baronet in 1869, with special remainder to his nephew.

JOHN STANISTREET was a native of Liverpool, the son of an eminent solicitor, who had an extensive practice, and was associated with some of the chief families there. He was, however, cut off in the prime of life—which, possibly, may have somewhat influenced the occupations and future destinies of his several sons.

The second son, John, very early exhibited considerable talent, which was especially developed in a decided taste for chemistry, electricity, and various departments of mechanics; leading him, amongst other contrivances, to the construction of an electrical machine and two small steam-engines. As he grew up, however, it became necessary for him to choose a profession, and he

adopted that of his father, whose surviving partner he ultimately joined. To this business he now devoted all his energy, following it with remarkable assiduity and ability, and quite forsaking with exemplary self-denial his former darling pursuits. As might thus have been expected, he assisted to raise his firm to the very top of the legal profession in Liverpool, and was employed in the negociation of some of the largest land sales ever effected in that great commercial city. Yet, in addition to the anxieties attendant upon this arduous pursuit of his profession, he had to suffer constantly, and occasionally severely, from a radically weak constitution, which became more manifestly developed in a serious disease of the lungs (apparently arising from a severe cold), which necessitated his passing a winter in Madeira, about the year 1840. He returned to England somewhat restored, but was ordered out again to pass the following winter there.

During Mr. Stanistreet's sojourn at Madeira on these two occasions he took several short voyages to the neighbouring islands, and went once in a Greek vessel up the Mediterranean as far as Greece. He thus acquired a taste for navigation, and during these voyages employed himself diligently in its study and practice, until he became able to determine the position of the ship by solar and lunar observations, in which he was aided by the possession of an excellent compensated duplex watch. By these means he obtained great skill in the use of the sextant, and on his return voyage he astonished the Greek captain by telling him on a certain day, that on the following morning he would see the island of Madeira—the prediction coming exactly true.

Unhappily, however, the main object of his voyages to Madeira was only very partially accomplished, his disorder being but slightly mitigated, and he continued to suffer great delicacy of health throughout the remainder of his life, making the toils of his profession peculiarly trying and laborious.

In 1851, in company with two friends, he went to Sweden, to observe the total eclipse of the Sun, and communicated an account of his observations to this Society. (*Monthly Notices*, Vol. XII., page 54.) He also proceeded to Spain in the *Himalaya* in 1860, with a view of witnessing the same phenomenon, but was disappointed by the unfavourableness of the weather.

A few years later he was attacked by another and more painful malady, which caused him frequent paroxysms of intense agony. All this was borne with exemplary patience and resignation, and, when not in actual pain, with surprising cheerfulness. But his increasing sufferings rendered his business-duties intolerably oppressive, and he was compelled, a few years ago, to retire from them, not however before he had realised an ample competency. Confined now almost absolutely within doors, he employed himself, as far as his remaining physical strength permitted, in cultivating, with more advanced skill, the mechanical and philosophical tastes of his youth. He converted a dressing-room of his house into a laboratory—furnished it with an excellent lathe,

an elaborate slide-rest, a dividing engine, a wheel-cutting machine, and even a small smith's forge, and there contrived and constructed various machines of great ingenuity. Chief among these were Mr. Stanistreet's A^1 clock, described and figured in the *English Mechanic* of March 8, 1872; and his micro-ruling machine, described and figured in the monthly journals of the Liverpool Microscopical Society for September and December 1871. The smallness, and regularity of rate, of the A^1 clock, in very unequal temperatures, was really a marvel. The compensation seemed perfect, and it is but inadequate praise to say that its going rivalled that of the best chronometers usually tested at the Royal Observatory.

Mr. Stanistreet's health, however, continued to decline, and after several attacks, from which his recovery seemed doubtful, he finally succumbed on the 17th of April, 1873. W. L.

Professor Giovan Battista Donati was born at Pisa, on December 16, 1826, and was the son of Dr. P. Donati, of that city. After his preliminary studies at the University of Pisa, where he had for his tutor our late Associate, M. Mossotti, he devoted himself to the special study of mathematics, and to original analytical researches. On August 1, 1852, he was appointed to the Observatory of the Museum of Natural History, at Florence, which at that time was under the direction of Professor Amici. In October 1854 he was made an *aide-astronome*; and on September 29, 1858, the title of *astronome-adjoint* was conferred upon him, in consideration of the discovery by him of the magnificent comet which still bears his name.

At this time Donati had already attracted the attention of astronomers out of his own country by his cometary observations, the results of which had been inserted in various scientific publications, including the *Monthly Notices*, where his name first appears as an observer and discoverer of comets in 1855. His fortunate discovery of a small telescopic comet on June 2, 1858, which ultimately became the remarkable comet of that year, brought him at once into general notice. From being a comparatively obscure observer, Donati found himself suddenly the astronomical hero of the day, for his brilliant comet not only formed an interesting subject for intelligent study, on account of the various speculations as to its physical constitution, but it also created for a time a lively taste for astronomy among all classes of the community. Fortunately, Donati's zeal as an observer did not allow him to remain satisfied with the honour he had so unexpectedly attained, for we find him continuing with increased energy as an assiduous observer of comets and other astronomical phenomena. On December 22, 1859, he was appointed Professor of Astronomy and Director of the Observatory.

On taking charge of the Observatory, Prof. Donati's first thoughts were directed to the advisability of constructing a new Observatory at Florence, because the situation of the building,

owing to the glare of the street-lamps and the continual vibration produced by passing vehicles, was not adapted for delicate observations of precision. Donati himself alluded to this defect in the position of his Observatory at a meeting on the subject of the Great European Triangulation, held at Berlin, in 1864. He was then most anxious that a new national Observatory for Italy should be erected, to take rank with the principal observatories of other countries, and his aim was to have the direction of an establishment which could be adapted to the present requirements of astronomy and terrestrial physics. To attain this object he laboured hard for many years, and it was a great day for science at Florence when, on October 27, 1872, the new Observatory at Arcetri was solemnly inaugurated with great ceremony, although, from the effects of an accident, Donati was unable to be present.

The original researches of Prof. Donati, which have eminently contributed to enrich astronomical knowledge with many new ideas and discoveries, embrace the four following subjects: Comets, Stella Spectra, Scintillation of Stars, and the Aurora Borealis. The observation of comets was, however, that which above all others was most attractive to him. From the commencement of his astronomical career to the end of the year 1864 he was the discoverer of six comets, on the following dates: June 4, 1854; June 3, 1855; November 10, 1857; June 2, 1858; July 23, 1864; and September 9, 1864.

Prof. Donati paid considerable attention to spectroscopic observations, and his labours in this branch of astronomy are well known by his important work, contributed in 1860 to the Museum of Florence, "Intorno alle strie degli Spettri Stellari," and continued in Vol. XV. of the *Nuovo Cimento*, 1862, and in the *Annali del Reale Museo di Fisica e Storia Naturale di Firenze*, 1865, New Series, Vol. I.

The experience gained by Donati from his observations of the spectra of the stars induced him to study the phenomena observed in their scintillation, and his more than usual clearness of conception enabled him to give an explanation of the causes which produce scintillation which he considered to be a true solution of the question; and in conformity with the views of Alhazen, who attributed the phenomenon to the variations in the atmospheric refraction.

Already Donati, in the first year of the existence of his new Observatory, had commenced a series of Notes, commencing with an account of some observations of the luminous phenomena of the great aurora of February 4 and 5, 1872. He deduced from the observations a general corollary that the phenomena observed during this great aurora were a series of successive formations from the east to the west, proportional to the difference of the longitude between the various stations from which they were observed, and consequently that they had a movement strictly connected with the apparent motion of the Sun. Admitting this fact, Prof. Donati was constrained to believe that the luminous

appearances of this aurora could not have been derived from a purely terrestrial, meteorological, or electro-magnetical phenomenon, but that they had a cosmical origin. Hence he has considered that these facts really belong to the domain of a new science, to which he has given the name of "Cosmical Meteorology."

In 1860 Donati published two papers under the title of "Memorie Astronomiche," one on the striæ of stellar spectra, mentioned above, and the second giving an account of his observations of the total eclipse of the Sun, made at Torreblanca, in Spain, in the year 1860. He was elected an Associate of this Society on November 11, 1864.

While on his return-journey from a visit to Vienna in September last, as the representative of his Government at the International Meteorological Congress, held in that city, Prof. Donati was attacked with Asiatic cholera. Although he was very ill at Padua, he was enabled to return to his home and family at Florence, but within a few hours afterwards he succumbed to the disease, deeply regretted by his numerous friends and co-workers in science. His death took place on the early morning (0.30 a.m.) of September 20, 1873, in the forty-seventh year of his age, at his residence, near the new Observatory at Arcetri, which he had laboured so long to establish, and which, it is hoped, will be his appropriate memorial for many generations.

E. D.

Matthew Fontaine Maury, the distinguished hydrographer, was born in Spottsylvania County, Virginia, on January 14, 1806. He was of French extraction. At an early age his parents removed into the neighbouring State of Tennessee, where young Maury received his education, but he ever retained a deep attachment for his native State. He first obtained a commission in the United States Navy as midshipman on board the *Brandywine*, at the age of sixteen; and in 1825 he was appointed to the *Vincennes* sloop-of-war, which had been commissioned for a four years' voyage, intended to include the circumnavigation of the globe, thus giving him an opportunity of improving his taste for observation in many climes and regions. After the return of the *Vincennes*, Maury served as "master" of the *Falmouth*, a ship stationed in the Pacific. It was during the many leisure hours which occurred on his four years' voyage in the *Vincennes* that he commenced his *Treatise on Navigation*, which afterwards became the adopted text-book on that subject in the United States Navy. After serving for a short time in the *Falmouth*, he was promoted to a lieutenancy on board the frigate *Potomac*, where his great knowledge of seamanship and his great scientific acquirements obtained such notice that he was selected in 1836 to accompany Captain Jones on an exploring expedition as director of the astronomical department. Owing, however, to some unforeseen circumstances, neither Maury nor Captain Jones sailed in the

expedition, both having sent in their resignations before its departure. In 1839, owing to an unfortunate accident, believed to have been a fall from a coach, which resulted in lameness, Lieutenant Maury was compelled to retire from active naval service afloat. The effects of the accident were very serious, and for some time it was feared that the valuable services of this enterprising scientific officer would be permanently lost to his country. The American Government, however, found for him a congenial office by placing him in charge of the depository of naval charts and instruments at Washington. Here his active mind soon succeeded in so reorganising the entire business of the institution that the name of the National Observatory was given to it, which was afterwards changed to the more appropriate designation of Naval Observatory.

Lieutenant Maury had from the time of his entry into the naval service a natural taste for meteorological observations, and more particularly for those relating to the winds and currents; and during his several cruizes he made it his frequent occupation to carry out several series of observations, under various circumstances, in order to form a theory of ocean meteorology, with the object of turning it to practical account for the benefit of mariners. When he became the directing mind at the depository of charts and instruments, or more properly the Hydrographic Office, he naturally used his great influence among naval men to recommend the regular observation at sea of the direction of the wind and of ocean currents, as well as other meteorological observations; the observer entering at the time his notes in the log-book, so that they might be available for a subsequent examination at the Observatory. The plan drawn up by Maury in 1842, and generally adopted in the service, included the registration of the direction and force of the wind every eight hours; the direction, velocity, depths, and limits of the various currents; the temperature of the air, of the water on the surface and at various depths, besides other phenomena bearing on the subject of ocean meteorology. In this manner a valuable collection of data was obtained, with the advantage that all the observations were made on one uniform system. The scheme answered so well, that in eight or nine years a sufficient number of observations had accumulated to fill 200 manuscript volumes, each averaging about 2,500 days' observations. It was understood that the master of every vessel, on returning from a voyage, was to deposit the "abstract log" at the Naval Observatory. The pith of this mass of records formed the material upon which Maury's celebrated work on the *Physical Geography of the Sea* was founded.

In this remarkable work Lieut. Maury gives a popular *résumé* of all his investigations relating to ocean meteorology. In the introduction his remarks on its contents explain, in a few words, the object he had in view in presenting the book to the public. "Under the term 'Physical Geography of the Sea,' will be included a philosophical account of the winds and currents of the sea; of the

circulation of the atmosphere and ocean; of the temperature and depth of the sea; of the wonders that lie hidden in its depths, and of the phenomena that display themselves at its surface. In short, I shall treat of the economy of the sea and its adaptations; of its salts, its waters, its climates, and its inhabitants; and of whatever there may be of general interest in its commercial uses or industrial pursuits." This work first appeared in 1855, and it gave an extraordinary stimulus to the study of ocean meteorology; but its great success did not arise altogether from the various theories contained in it—some of which have been objected to, especially that explaining the Gulf Stream—but in a great measure its popularity was owing to the interesting manner with which the driest of meteorological details are arranged and discussed. The work has not only been republished in several editions in England, but it has also been translated into all the principal European languages. Adopting the words of a recent writer: "His unrivalled powers of application and untiring perseverance were combined with rare gifts of imagination, and an almost poetic style. Hence he not only collected materials with judicious discrimination, and arranged them with critical skill, but also combined them so as to produce one of the most fascinating books in the English language."

In 1853 Lieut. Maury represented the United States at the Maritime Conference held at Brussels in that year, for devising an uniform system of meteorological observations at sea. Most of the maritime nations of Europe were also represented at this Conference, including England, France, Russia, Belgium, Holland, Portugal, Denmark, and Norway and Sweden. This international scientific meeting was brought about principally through the exertions of Maury, who was one of its leading members, his advice and labours calling forth from his colleagues an expression of thanks for the enlightened zeal and earnestness he had displayed in the important and useful work which formed the subject of their deliberations. One of the principal results of the Conference was the establishment in 1855 of the Meteorological Department of the Board of Trade, which was placed under the direction of the late Admiral Fitzroy.

When, in 1861, the outbreak of civil war between the Northern and Southern States of America occurred, Captain Maury, like many other high-minded Virginians, threw up his commission in the Navy of the United States, choosing rather to be considered a rebel than to fight against his kinsmen and friends of his native State. No one can doubt that his sacrifice even of professional advancement was great, but his loss to science was greater, by this sudden resignation of his position at the Naval Observatory. But Maury was a Southerner by birth, and his national feeling of honour would not allow him to remain in the service of those whom he considered to be the determined enemies of those most dear to him. Hence on April 19, 1861, he threw in his lot with the Confederate States of the South,

the Government of which he assisted in various ways, especially by his counsel in the construction of its maritime defences, which enabled the South to hold their own so long. During the war his house was destroyed, together with a valuable collection of manuscripts and books; his scientific instruments were confiscated, and he came to Europe on a visit as an impoverished exile. He was received in Europe with every mark of sympathy and respect; and at a banquet given in his honour a valuable testimonial, consisting of a purse containing upwards of 3,000 guineas, was presented to him by Sir J. S. Pakington, in the name of his friends of all nations. While in Europe many attempts were made by more than one Government to attach him to their service, but he remained faithful to his own State till the final overthrow of the South, when he joined the fortunes of the Emperor Maximilian, in Mexico, who appointed him Imperial Commissioner of Emigration. He remained in this office till the fall of Maximilian, when he retired to Virginia, taking the position of a Professor at the College at Lexington. During the last few years of his active life Maury, though in frequent ill-health, exhibited the same devotion and energy in all that he undertook as in his earlier days. One of his latest wishes was to organise a system of combined meteorological reports on land for the benefit of farmers as well as of sailors. He was anxious that his fellow-citizens should discard political animosity, and devote themselves to the development of the agricultural and other resources of their country. He remained thus to the end of life the friend of science and his fellow-man, and he has passed away leaving many warm and attached friends on both sides of the Atlantic. Captain Maury died at Lexington, on the 1st of February, 1873, having a few weeks before completed his sixty-seventh year. He has been an Associate of this Society since January 12, 1855. E. D.

Proceedings of Observatories.

The Council have received the following Reports from the Directors of the principal British Observatories.

Royal Observatory, Greenwich.

The changes to be recorded during the past year are for the most part unimportant, the usual observations having been made with the usual instruments. Among the stars observed for a special purpose was Schjellerup 241, to which Mr. Birmingham directed attention: a change of brightness in this interesting object from the 5th to 7th magnitude was noted with the Transit Circle in the short space of a fortnight, though a meridian instrument is hardly adapted to such an inquiry. Tempel's Comet was observed with the Great Equatoreal in May last; the results have been communicated to the Society, and will be found in the *Monthly Notices*. No certain markings have been detected on *Venus*, *Uranus*, or *Neptune*, though the former was examined with the Great Equatoreal on every favourable opportunity during the approach to, and passage through, inferior conjunction.

For some years past a discordance has been remarked between the results obtained for coincidence of the two collimators of the Transit Circle when the instrument was raised so as to allow of a perfectly free view, and those when a partial view was obtained through the pierced cube of the Transit Circle. A correction to the collimation error of $0''\cdot28$ deduced from the observations of the past three years, has been applied from January 1, 1874, in all those cases where the pierced cube was used. The effect of this is to alter the Level and Azimuth errors slightly, and to increase the apparent clock-error by $0^s\cdot07$, but the correction to the resulting R.A. of stars is in the great majority of cases quite insensible, the effect being entirely differential. The recording micrometer described in the *Monthly Notices*, Vol. XXXIII. p. 484, has been in use since June 20, 1873, and has enabled the system of making several bisections of an object in its passage through the field to be carried out effectually; mistakes have in some instances been corrected by its means.

Since the extensive repairs at the end of 1872 the Altazimuth has worked more satisfactorily.

The Astronomer Royal has arranged a barometric compensation for the Sidereal Standard Clock, in which a magnet carried by the pendulum is acted on by another magnet, which is brought nearer to, or carried farther from it, by the float of a siphon barometer. The clock Arnold I. has been used during the adaptation of this arrangement.

Owing to the extensive use of the Greenwich Time Signals, which is now made for longitude determinations and other purposes, it appeared desirable to test the action of the Chronopher at the Post-office, used for transmitting the signals to all parts of the country by relay action. This was done last September, when it was found that the signal sent through the Chronopher was received without the slightest appreciable loss of time.

An addition has been made to the instruments exhibited to the public at the Entrance Gate in the shape of a balance of peculiar construction for testing pound-weights. Though accessible to the public, the instrument has not received the slightest injury.

Some small alterations have been made in the Great Equatoreal, in view of the spectroscopic observations which will be commenced shortly. The construction of the Spectroscope has been much delayed, but it is now nearly completed, and will be brought into use without loss of time. An induction coil capable of giving a 6-inch spark to be used in connection with it has been procured. The observations will be made by Mr. E. W. Maunder, who has been appointed spectroscopic and photographic assistant. Photographs of the Sun have been taken at every available opportunity since June 1, 1873, either with the Kew or one of the new photoheliographs, and an extensive series has already been formed, about 200 negatives having been preserved up to the present time. Some of these possess a peculiar interest, from the circumstance that on two occasions photographs were obtained of a spot on the actual limb of the Sun, presenting the appearance of a notch partially filled with less luminous matter. The structure shown on the photosphere in several of the negatives is very remarkable, not only in itself, but also as showing the superiority of photography to eye observation.

The volume of *Observations* for 1871, with an Appendix containing a history and description of the Water Telescope, is published, and has been distributed. The distribution list was much larger than usual, chiefly through the incorporation of the list of the Royal Society, which has now ceased to distribute any of the Greenwich publications.

Radcliffe Observatory, Oxford.

No change whatever has taken place in the organisation of this Observatory during the past year, nor in the nature of the work which is being carried on; and the preceding Report made to the Annual General Meeting for 1873, may be referred to for a statement of the number of assistants at present employed, and for other details.

The meridional work has been carried on as heretofore with as much vigour as the weather would allow, and the same attention

has been paid to the observations of occultations of stars by the Moon, and to the phenomena of *Jupiter's* satellites, the results of these observations up to June 1873 being printed in the *Monthly Notices* for that month.

Considerable progress has been made in the reductions of the observations, but chiefly in the most oppressive portion of them, namely, in those relating to the N.P.D. of stars. With the exception of the corrections to mean N.P.D. of a portion of the stars, the reductions in N.P.D. are complete for 1872, as well as for 1871, and the zenith-points are formed for the greater portion of 1873.

The meteorology is still not so forward as could be desired, though considerable progress has been made.

The volume for 1870 was finished and distributed in the autumn of last year, and the catalogue of stars for 1871 is now being printed, so that the astronomical portion of this volume will probably be completed in two months. The separate mean right ascensions for 1872 have also been printed for some time, and the N.P.D.'s will shortly be ready for press.

With the Heliometer the usual amount of work has been done, chiefly in double stars; and Mr. Bellamy has been acquiring experience in this class of observations, and in the general management of the instrument and its adjustments.

It is necessary also in this Report to make mention of the progress of the printing of Sir J. Herschel's posthumous Catalogue of Double Stars, entrusted by the Council to Professor Pritchard and the Radcliffe Observer, and read and revised under the immediate charge of Mr. Keating. As all the stars are compared with the original authorities, this is a work of considerable labour, and necessarily occupies a great deal of time. Its progress therefore has not been rapid, but it is now approaching its completion, about three-fourths of the whole catalogue having been revised and printed.

Savilian Observatory for Astronomical Physics, Oxford.

A full description of this new establishment has already been given in the *Monthly Notices* for December. It will be, it is anticipated, in active operation before the fall of the year. The contrivances for the domes appear to be promising, and are certainly very ingenious; if they prove to be successful, they will probably be generally adopted where wide and continuous shutters and great facility of rotation are objects of importance. Mr. P. P. Baly, C.E., of 6 Victoria Street, Westminster, is the engineer of this very important part of the structure.

The Professor repeats the intimation which he has already made, as to the acceptability of the publications of other great

observatories, towards the formation of an astronomical library for this new establishment.

Cambridge Observatory.

The astronomical observations have been almost exclusively confined to Zone observations made with the Transit Circle, in pursuance of the arrangement with the German Astronomische Gesellschaft.

The mode of observation described in the last Report has been found to work so satisfactorily that no alteration has been made in it, and its efficiency can be judged of by the fact that during the year 5,968 observations of small stars have been made, in addition to 602 of standard stars for clock corrections, and 158 of slow stars for instrumental corrections.

As the clock stars are taken in or near the Zone, instrumental errors have very little effect in the reductions of the Right Ascension observations. Notwithstanding this, and the very great stability of the instrument, the level and collimation errors are determined several times every week, and the azimuthal error as often as suitable stars can be obtained.

The nadir point is observed on nearly every day favourable for the observation of stars, and is used in the reduction of the Zones, although it is intended ultimately to refer the declinations of the small stars to those of the standard stars employed by the Astronomische Gesellschaft, and thus to make the Cambridge Catalogue homogeneous with the kindred catalogues of other observatories.

Some progress has been made in the reductions, but of course cannot nearly keep pace with the observations. The latter will, however, become less numerous in a year or two, and there will then be some hope of gaining upon them.

The meteorological observations are made as usual and communicated daily to the central meteorological office in London. Besides this it has been arranged with America to make simultaneous observations every day at $0^h\ 45^m$ Greenwich Mean Time, to be forwarded to London every fortnight.

A Kew Standard Thermometer, ranging from 5° to 225° Fahrenheit, has been procured for the Observatory by means of the Sheepshanks Fund.

Royal Observatory, Edinburgh.

The work here has consisted chiefly in daily time signals by ball, gun, and controlled clocks; meteorological reductions from 55 observing stations, and star-catalogue computations.

The new Equatoreal is not yet out of the contractor's hands, and H.M. Office of Works is corresponding with him on the subject.

The Second Assistant resigned and left the Observatory last November; and as H.M. Government have not yet appointed a successor, the Astronomer and his now sole Assistant, Mr. Alex. Wallace, are unequal to undertake all that *is* desired, and *has* formed the rule of the establishment for many years past.

Dunsink (Dublin) Observatory.

The observations of double stars with the large Refractor of this Observatory have been continued during the early part of the past year, until the arrival of the new Meridian-circle, made by Messrs. Pistor and Martins, in Berlin, which was mounted last May. This instrument, though planned by Mr. Martins, has been executed, after his untimely death, by Mr. Pistor; and Dr. Brünnow remarks that it gives great satisfaction in every respect, and compares very favourably with other instruments of the same kind made before by that distinguished firm. The investigations which have been made in regard to the instrumental errors give the most satisfactory results, and show that the accuracy of the workmanship and the steadiness of the instrument are most perfect. Dr. Brünnow has been for some time in bad health, and besides he has been working without an assistant since last spring, so that he has not yet been able to examine the divisions of the circles; but the observations of standard stars hitherto observed show that their errors must be very small. During the opposition of *Flora* in October and November this asteroid has been compared in declination with the stars selected by Professor Galle, with a view of determining its parallax. Observations have been obtained on eleven nights, the results of which have been communicated to Professor Galle.

The comparisons of the planetary nebula in *Draco*, H. IV. 37, with a neighbouring star have been reduced. The computations made in order to determine its parallax give no appreciable value for it, the actual result being a parallax of $0''\cdot05$, with a probable error of $0''\cdot03$.

Durham Observatory.

In accordance with the notice given in the last report, the work of making extra-meridional observations of the minor planets has been discontinued at Durham during the past year, but it is

hoped that it may be partially resumed shortly. A good series of observations of Comet iv. 1873 has been obtained; and though some delay has occurred in the publication of the results, they will be ready for the press immediately. A few isolated observations of other comets have also been taken.

The spectroscope has been successfully applied to the analysis of the light of the before-mentioned comet, and the observation has been published through the medium of the *Bulletin International* of the Paris Observatory. The light of the comet was found to yield a carbon spectrum entirely similar to that of Winnecke's comet of 1868.

A series of measures of the diameter of *Venus* was made in the spring of the year with the Double Image Micrometer. These observations and the resulting determination are to be found in the supplementary number of the *Monthly Notices*. The indication they afford of the variation in the amount of irradiation with the transparency of the atmosphere was quite unexpected, and may perhaps be considered to be of some interest.

The usual meteorological observations have been continued morning and evening, and, together with the deduced results, are now communicated monthly to the office of the Meteorological Committee of the Royal Society. The meteorological work has also been somewhat increased by the observer taking part in the scheme for synchronous observations of weather over the whole of the northern hemisphere, which has been suggested by American meteorologists, and which was commenced at Durham on January 1, 1874.

Glasgow Observatory.

The operations at the Glasgow Observatory during the past year, exclusive of the usual routine work, have consisted mainly in the reduction of the star observations which have been made with the Transit Circle in previous years. These observations are still being prosecuted, but it is intended that they shall be brought to a close in the year 1875. They comprise the right ascensions and north polar distances of a selection of stars included between the sixth and ninth magnitudes, extracted chiefly from Bessel's Zones. The results will then be employed in the construction of a catalogue of stars.

Kew Observatory.

During the past year Mr. Whipple has been engaged, under Mr. De La Rue's direction, in measuring the faculæ depicted on the

solar photograms obtained during ten years at this Observatory. The work has been completed up to March 1866. Some unforeseen interruption has occurred in the measurements and discussion of the Sun-spots, which will cause a delay of two years in the publication of the results. Arrangements are being made for the micrometrical measurements to be conducted at the Kew Observatory, and the work will be completed as soon as possible by Messrs. De La Rue and Balfour Stewart.

Liverpool Observatory, Bidston, Birkenhead.

At this Observatory, during the past year, between four and five hundred chronometers have been tested for the Merchant Navy, in the three temperatures 55°, 70°, and 85°. The object of testing these instruments in three definite temperatures is to supply the mariner with the means of calculating the amount of change in the rate of his chronometer due to error of thermal adjustment. As a rule there appears to be a definite temperature peculiar to each chronometer, in which the instrument goes faster than in any other temperature; and as the number of degrees above or below this temperature of maximum gaining rate increases the chronometer loses in a rapidly increasing ratio. Assuming this law of variation to be that the change of rate is directly as the square of the number of degrees from the temperature of maximum gaining rate, the rates calculated on such assumption are found sensibly to agree with those obtained from observation. By giving the rate of each instrument in three temperatures at equal distances from each other, the calculations are rendered very simple for finding the temperature (T) in which the chronometer has its maximum gaining rate, (R) the rate at (T), and (C) the factor, which multiplied by the square of any number of degrees from (T) shows the amount of loss for that number of degrees. The temperature of maximum gaining rate in most instruments is found to be between 60° and 80° of Fahrenheit, but in some it is below zero, and in others above one hundred degrees. The value of the factor (C) has been found to range from 0·001 to 0·006, but there are a large number of chronometers in which it is found to be about 0·003. From this it appears that the average correction for error of thermal adjustment at 15° from (T) = $-$ 0^{s}·67, at 30° = $-$ 2^{s}·70, and at 45° it amounts to $-$ 6^{s}·08. By repeating the test at distant intervals of time (T) and (C) are found to remain sensibly the same for long periods, but (R) is more changeable, and should be re-determined on all favourable occasions. The factor (C) is the measure of the degree of perfection with which the balance compensates for the change of elasticity in the balance-spring, and chronometer-makers are much interested in seeing the value of this factor. For any chronometer which has been allowed to

remain at the Observatory for a period of five weeks the certificate of test issued with the instrument contains the necessary data for calculating (C), (T), and (R).

Stonyhurst Observatory.

At this Observatory the 8-inch Equatoreal has been devoted chiefly to observations of *Jupiter's* satellites, and to solar photography. The partial eclipse of May 25, 1873, offered a favourable opportunity for work, as the sky was clear from the beginning until past the middle of the eclipse. Good pictures of Sun-spots have been also obtained both with the pyrogallic and with the iron developer, and some time has been devoted to solar spectroscopy, but since April last a considerable portion of the work has had a direct bearing on preparations for the approaching transit of *Venus*. A new self-recording micrometer was also designed by the mechanical assistant.

The usual photographic records of all the principal meteorological and magnetic instruments have been continued without intermission, so as to render this establishment as complete as possible in all the details of a physical observatory. The results of a magnetic survey of Belgium, made with the Stonyhurst instruments, have appeared this year in the *Philosophical Transactions*.

Temple Observatory, Rugby School.

The chief work of this Observatory during the past year has been the measurement of double stars, the distances and positions of some 430 having been obtained by Mr. Wilson and Mr. Seabroke. In this work they have been assisted by members of the school, some of whom show great interest in observatory work. A graphical solution of the orbit of Σ 1938 was effected by Mr. Wilson.

The solar prominences have been carefully observed, principally by Mr. Seabroke, on nearly every day on which the Sun was visible, and drawings made of the Chromosphere on a scale of 2 *mm.* to each degree of the Sun's circumference. Drawings have been made on 86 days. For all the observations the 8½-inch Alvan Clark, furnished with the ring-slit arrangement described in *Proceedings of the Royal Society*, January 1873, and a spectroscope giving a dispersion of 8 prisms of 60 degrees, have been used.

Some attempts have been made by Mr. Seabroke to compare the spectra of the two components of *Castor*, and to follow the variations in the spectrum of *Algol*.

Mr. Seabroke has made some experiments on the heat and light-radiating powers of gases at low pressures, with reference to a theory of molecular vibration.

A few drawings of Sun-spots and Planets have been made by members of the school.

The visitors to the Observatory who have made use of the telescope during the present year have numbered 383.

Cranford Observatory.

The Observatory which Mr. De La Rue erected at Cranford,* in 1856, was dismantled in June last, and the large reflecting telescope and several other instruments which it contained have since been presented to the University of Oxford, towards the equipment of the new Physical Observatory founded by that body, and now in course of erection under the direction of Professor Pritchard.

Mr. Barclay's Observatory, Leyton.

During the past year the regular and systematic measures of double stars have been carried on, as well as the observations of comets, eclipses, occultations, and phenomena of *Jupiter's* satellites. The third volume of *Observations* has also been published, and distributed to the principal observatories and scientific men, both at home and abroad. It is so strongly felt that the great value of scientific work is its continuity and early publication, that the temptation to take up new and most interesting branches of astronomical physics has been resisted.

Mr. Bishop's Observatory, Twickenham.

During the last year attention has been chiefly directed to the completion of the Ecliptical Charts, though progress has been much interrupted by the unusually cloudy state of the weather in this locality in the absence of moonlight, when alone the chart-work can be effectually proceeded with. The two charts for Hour 6 of R.A., where the telescopic stars are exceedingly numerous, are approaching completion, and it is also hoped to engrave

* Mr. De La Rue requests us to state that he has now finally left Cranford, and that his new address is 73 Portland Place, London, W.

those for Hours 12 and 15 in the course of the ensuing spring. The division of the 6th Hour of R.A. in two charts became necessary, in order to show on a sufficiently clear scale the large number of stars included within three degrees from the ecliptic in this region.

The vicinity of Tycho's star in *Cassiopeia* (*Nova* 1572) has been closely watched since the autumn of 1870, but particularly during the last twelvemonth. It is well known that another *maximum* of this remarkable object has been anticipated about the present epoch, though, admittedly, on somewhat uncertain grounds. Nevertheless, the Observatories of Pulkowa and Copenhagen have given attention to the telescopic stars in the vicinity within the last ten or twelve years. The frequent comparisons made by Mr. Hind and Mr. W. E. Plummer in 1873 have left no doubt of a sensible fluctuation in the light of the star numbered 129 in Professor D'Arrest's Catalogue, communicated to the Royal Society of Sciences at Copenhagen in January 1864, though the extremes hardly include a whole magnitude. This star is within one minute of arc from the position assigned by Professor Argelander to Tycho's star, from a very careful discussion of his measures of distance from neighbouring stars, included in *Astronomiæ Progymnasmata.* It has frequently presented a more blurred or hazy appearance than the *vicinæ*, and on several nights, particularly last January, has been remarked to flash up very sensibly for moments, assuming at these instants a redder tinge than at other times. A suspicion of variability has also arisen in the case of D'Arrest's No. 175, but this star is too far from Argelander's position to be mistaken for the *Nova* of 1572. The star assumed to be Tycho's will be readily identified by means of a bright ninth magnitude—8·9 according to Argelander, which is No. 22 of his Zone 60; it follows this ninth magnitude $29^s \cdot 60$, and is south of it $10' \, 4'' \cdot 1$ by micrometrical comparisons made at Twickenham in August 1872.

Several red and variable stars not included in the catalogues of Schjellerup and Schönfeld have been remarked in other parts of the heavens, particulars of which will shortly be communicated to the Society.

Observations for position have been made of the following comets: Tempel's first comet of short period, May 16 to 30; his second comet of similar period, detected 1873, July 3, from August 2 to October 20; this comet was seen till October 26, but it was too faint for satisfactory observation on this night; Paul-Henry's comet, from August 26 to September 12. Several places of Borrelly's comet were also obtained. The periodical comet of Brorsen was observed by Mr. Plummer from September 17 to October 26; it was much brighter than anticipated in October.

In astronomical calculations, the circumstances of the return of two comets have been determined by Mr. Plummer. The first of these—Tempel's of 1867—was roughly computed, more with the view of discovering whether the exceedingly heavy perturba-

tion, which Mr. Hind had remarked the comet must suffer from near approach to the planet *Jupiter*, about the last aphelion passage, would tend to bring the comet into perihelion at that time of the year, when alone, from the situation of its orbit, it can be favourably placed for observation, than to supply an accurate prediction of elements. The approximate results obtained by Mr. Plummer induced Dr. von Asten, of Pulkowa, to repeat the calculations rigorously. The other comet of which the return has been calculated is that of Brorsen (first detected in February 1846). The perturbations by *Jupiter* and *Saturn* were determined for forty-day intervals, and an ephemeris computed from the resulting elements, which, having been communicated directly to M. Stéphan, Director of the Observatory at Marseilles, that energetic astronomer was enabled thereby to detect the comet on August 31. The predicted time of perihelion passage was about half a day later than the true one. Parabolic elements have been computed for the comets of Borrelly and Paul-Henry, which have appeared in the *Monthly Notices*. Some time has been occupied upon the comet of 1812, detected by Pons on July 20, and subsequently independently discovered by Wisniewsky. As is well known, Encke (*Zeitschrift für Astronomie*) deduced an ellipse with a mean distance of 17·09549, but subject to a very considerable probable error. On this account Mr. Hind suggested to Mr. Plummer that, by using improved solar tables, and reducing such observations as we possessed in the original form with the best star places, the period of revolution might possibly be ascertained within narrower limits. Mr. Plummer accordingly calculated a solar ephemeris from M. Le Verrier's tables, and newly reduced the series of observations by Bouvard in the first folio volume of *Paris Observations*, and a series at Viviers by Flaugergues, published by Zach in his *Correspondance Astronomique*, t. V., which Flaugergues considered "very exact," and which does not appear to have been available to Encke. The places of the comparison stars have been generally carried back from the later Greenwich Catalogues. The further reductions were made with every precision. As a first attempt to judge how far Encke's orbit admitted of improvement, three normals were formed and compared with positions computed direct from the elements with Le Verrier's position of the Sun. The agreement was found to be even closer than exhibited by Encke's comparison with his normals (*Zeit. für Astron.*, II. p. 393). This circumstance appears to indicate that very little improvement upon Encke's orbit can be effected. In this state the work at present remains.

Mr. Bishop has now in the press the differences in Right Ascension and Declination between Comets and Stars of comparison in observations taken since the re-erection of his Observatory at Twickenham, having been induced to put these upon record through several applications being made for them by astronomers engaged in the calculation of definite orbits.

The results of computations bearing upon the circumstances

of total eclipses of the Sun available for physical observations before the close of the present century, are also preparing for the press.

Mr. Huggins' Observatory, Upper Tulse Hill.

Since the last report, in February 1872, the observations on the motion of stars in the line of sight have been extended to 31 stars, of which 11 were found to be approaching and 20 receding at velocities from 15 to 50 miles per second. The detailed results will be found in the *Proceedings of the Royal Society* for 1873.

New comparisons of the first line in the spectrum of the nebulæ with the brightest line of nitrogen, show that the nebular line is single and defined, while the line of nitrogen is double and each component nebulous. The line in the spectrum of the nebulæ appears to be coincident with the middle of the less refrangible of the lines. The wave-length of this line on Ångström's scale will be 500·4. The second line appears to be coincident in position, or very nearly so, with a line in the spectrum of iron. The wave-length of this line is 495·7. The third and fourth lines, which are probably present in the spectra of all the gaseous nebulæ, coincide with the lines β and γ of the spectrum of hydrogen, and indicate the presence of this substance in the nebulæ.

Observations on the motions of the nebulæ were made possible by the circumstance, that a single and defined line in the spectrum of lead was found to be sufficiently coincident with the first line of the spectrum of the nebulæ to be used as a fiducial line of comparison. Repeated observations on different nebulæ show certainly that these bodies are not moving with the same velocity as some of the stars.

In 1872 several sets of measures, with a spectroscope of ten prisms, through which the light passes twice, were made to determine the wave-length of the bright line D_3 in the red matter round the Sun. The result came out 587·48 on Ångström's scale, or in wave numbers 1702·18.

Measures of the lunar crater *Linné*, taken in 1873, when compared with measures taken in 1867, seem to show that no permanent change has taken place on the Moon's surface at that spot during the last nine years. A detailed account of these observations is inserted in the January number of the *Monthly Notices*, vol. xxxiv., p. 108.

Lord Lindsay's Observatory, Dun Echt, Aberdeenshire.

The 6-inch and 15-inch Equatoreals, the Transit Circle, the Foucault Siderostat, and the Heliometer, by Repsold, mentioned in last year's Report, have arrived from the makers, and been successfully erected and adjusted.

The original wheels and rails of the great Equatoreal Dome have been removed and replaced by a live-ring similar to that contrived and carried out by Mr. Grubb at the Observatory, Dunsink, and described by Dr. Brünnow in his first volume of *Astronomical Observations and Researches made at Dunsink.* The result is, that whereas two men were formerly required to move it, now the whole dome of 24 feet in diameter, and weighing nearly five tons, can be completely turned round with the greatest ease in one minute, a pull of 6 lbs. only being required to set it in motion. The shutters have also been improved in their ease of action and closeness, and are now all that can be desired. During the early part of the year a number of observations were made for latitude with the portable Altazimuth; the stars were selected in pairs of nearly equal and opposite zenith distance. It was found that the north stars gave invariably a larger latitude than the south stars, the discordance following very nearly the law of the sine of the zenith distance. Flexure of the telescope alone would account for this, but an ellipticity of the circle also was suspected, and the instrument has been returned to Mr. Simms, to have four microscopes applied instead of two. The result of a discussion of the observations gives for the latitude $57^\circ\ 9'\ 35''\cdot 2$ as compared with $57^\circ\ 9'\ 36''$ deduced from observations of *Polaris* alone.

The greater part of April and May was occupied in York, London, Paris, and Dublin in superintending the completion of the various instruments. June and July were entirely occupied with the erection of the live-ring of the dome, the 6-inch equatoreal, the heavy parts of the great equatoreal, and the adjustments of the collimators of the transit circle.

In the end of July Mr. Gill left for Pulkowa to visit the Observatory and arrange for co-operation with Russia in heliometric observation of the Transit of *Venus*—the Dun Echt heliometer being precisely similar to the Russian instruments. Lord Lindsay and Mr. Gill were also present at the meeting of the Astronomische Gesellschaft in Hamburg, and at the meetings of the German Committee for the Transit of *Venus* at Hanover. The result was to facilitate a co-operation with America (represented by Professor Simon Newcomb and Professor C. H. F. Peters, of Clinton), in photographic observation (with telescopes of long focus) of the Transit of *Venus*; and also the acquisition of much practical information on their experience in the use of the heliometer from Professor Auwers and Professor Winnecke.

On their return in September Mr. Grubb arrived with the object-glass and some of the smaller fittings of the Great Equatoreal; these were duly put up and adjusted. The object-glass appears to be a very excellent one. In October the Astronomer's house was completed, about the same time the heliometer arrived from Messrs. Repsold. It has since been constantly employed in a determination of the division errors of its scale, the thermal coefficient, the absolute value of the scale, the change of focal length of object-glass by temperature and the position of the focal point. For division-errors of the scale forty-eight divisions on each scale have been selected, equivalent to a space of about 40′.

Double webs approximately at intervals of 12 and 6 divisions have been inserted in the micrometer frame, and the scales successively divided into 4 and 8 parts. About 4,000 measures have already been made in this investigation, and the division-error of 8 points on each scale determined, with a probable error of less than 0″·02. The results show that the scales are very beautifully divided, the mean error of a division (of the divisions yet examined) hardly exceeding 0″·10.

The distances and position angle of the brighter components of the *Pleiades* from η *Tauri*, as well as many of their mutual distances, have been observed for comparison with Bessel's observations of the same objects. The distance between g and f *Pleiadum* (16 and 27 *Tauri*) = 61′ has been measured on nearly every night of observation, as a mode of obtaining a value of the temperature coefficient. For absolute value of the scale, measures have been commenced on two zones of stars furnished by the kindness of Dr. Auwers, a north zone containing five stars, and a south zone containing ten stars. The intervals of these stars, as determined by the heliometer, are projected on the great circle joining the extreme stars, and the result so obtained is compared with the distance between the extreme stars as determined by meridian observations. The same zones are being measured by the German observers. The change of focal length of the object-glass by heat has been found by a series of 86 observations to be almost exactly equal to that of the brass tube of the instrument.

In the beginning of December the Transit Circle was erected. The collimators, and the standards carrying the segmental bearings of the transit pivots, having been previously adjusted by a small telescope mounted on an axis carrying pivots similar to those of the Transit Circle itself, the work of mounting was very rapidly accomplished, and the instrument found sufficiently near in azimuth and level. There is no adjustment for either of these errors.

This Transit Circle being almost perfectly similar to the Cambridge instrument, by the same makers, one of the first tests to which it was put was to ascertain whether it had the same error as that described by Professor Adams in the last Annual Report of the Cambridge Observatory (*Monthly Notices*, February 1873), viz.: "When the instrument is turned through the south horizon

to the Nadir the reading of the observed Nadir point is about $1''\cdot 5$ in excess of that which is obtained when the instrument is turned through the north horizon to the Nadir."

Mr. Simms feared a buckling in the side of the cube to which the cones of the instrument are attached, and accordingly the axis of the Dun Echt instrument was made considerably stronger, and with interior stays cast with the cube to stiffen the instrument as much as possible.

The following are the results of measures for this error:—

Date	Seconds of Nadir Point Through North	Seconds of Nadir Point Through South	Excess when turned through South
	$''$	$''$	$''$
Dec. 17	50·85	51·27	+0·42
Dec. 18	51·76	53·37	+0·61
Dec. 27	7·71	8·32	+0·61
Dec. 30	15·50	15·99	+0·49

These results seem to show the reality of a similar error, though of smaller amount. The discordance of seconds of Nadir point is accounted for by process of adjustment of the microscopes in the intervals between the observations. The instrument is provided with the means of exchanging the objective and eye end, and of rotating the micrometer so that a complete investigation of the source of the error is believed to be possible, and will be undertaken so soon as more pressing investigations will permit. An apparatus for investigation of the flexure at every altitude has been contrived, and is in course of construction. The pivots have been submitted to examination, though the investigation is yet very incomplete; it is sufficient to show that the errors are excessively small, and probably may be neglected. Measures of the radius of curvature of the siderostat mirrors have been commenced.

The 4-inch object-glass, of 40 feet focal length, has been received from Mr. Dallmeyer, and Mr. Grubb reports that the Cassegrain reflector is ready.

The transportable dark rooms and Photographic Observatory, for the Transit of *Venus*, have been designed; they are far advanced in construction, and will be erected for trial in February. In the meantime a large number of experiments on photographic processes have been carried out in the laboratory in London.

The methods for determining the absolute values of angles on the photographic plate, and the elimination of contraction of the photographic film, are worked out.

Since the erection of the Transit Circle a six-pounder cannon, charged with 2 lbs. of gunpowder, has been fired daily as a time-signal, and has been found a great boon to the district for many miles round, as previously no standard of time existed.

It is regretted that the frequent necessary absence of Lord Lindsay and Mr. Gill from the Observatory, the pressing helio-

meter work, and the large amount of detail to be worked out in connection with the Transit of *Venus*, have rendered it impossible to undertake the intended observation of the phenomena of *Jupiter's* satellites.

Lord Lindsay has engaged the services of Mr. Henry J. Carpenter (for many years a computer at the Royal Observatory), who will soon take up his residence as First Assistant, and will remain in charge during the absence of Lord Lindsay and Mr. Gill in the Mauritius, for the observation of the Transit of *Venus*.

Mr. Lockyer's Observatory.

This Observatory has been especially devoted to a study of solar phenomena by means of the Spectroscope; and as such observations are in Mr. Lockyer's opinion useless unless accompanied by laboratory researches, a laboratory has been established in connection with it. In both laboratory and observatory work Mr. Lockyer has been aided by Mr. R. J. Friswell.

The routine work is as follows:—

1. The chromosphere is sketched upon every available opportunity, and the shapes and heights of the prominences, together with the character of the chromosphere noted; and when possible the direction of the inclination exhibited by its filaments or tongues are recorded. In order to increase the chances of obtaining chromospheric drawings, the co-operation of Mr. G. M. Seabroke, of Rugby, was secured, so that bad weather in London might be compensated by possibly good weather at Rugby.

2. Spectroscopic observations of Sun-spots: the metals whose lines have been either thickened or distorted, or both, being noted; the hydrogen lines have been watched for bright reversals, &c.

3. Photographs of the spectrum of the Sun and of the solar metals.

4. Reduction of photographs.

In the chromospheric observations the following instruments have been used:—

A 6-inch refractor, by Cooke of York, mounted equatoreally.

A 7-prism spectroscope.

A position circle, attached to the eye-piece end of the telescope, for obtaining the position angle on the Sun of the prominences observed.

Since September 1873 the 7-prism spectroscope has been replaced by one of Mr. Lewis Rutherfurd's magnificent speculum metal diffraction gratings. This is lightly mounted, with an observing telescope collimator and slit of the usual sort, and far surpasses the 7-prism instrument in lightness, handiness, and

ease of manipulation, and it is also spectroscopically much better, admitting more light with the same amount of dispersion.

The days unsuitable for solar observation have been employed in researches on numerous points of spectral analysis, with a view to the application of the results obtained to the study of the Sun and stars.

Attention was at first devoted to an investigation of a fact noticed by Dr. Frankland and Mr. Lockyer in 1869—namely, that when an image of a spark from a metallic electrode was thrown on the slit of a spectroscope the lines in the spectrum were of different lengths. This was found to be the case with all metals, and as soon as this was ascertained the following were mapped: Li, Na, Mg, Al, Mn, Co, Ni, Zn, Cd, Sr, Ba, Sn, Sb, Pb, showing the long and short lines.

It was observed that certain metals enclosed in glass tubes, and subjected to a continually decreasing pressure, lost their short lines first, the longest lines remaining longest visible.

Also when the spark spectra of chlorides were observed it was noticed that, as a rule, only the longest lines of the metal were visible, the shorter ones being either entirely absent, or appearing as dots on the pole.

Hence it appeared that only the purest and densest vapours gave the complete spectrum, and that dilution caused the shortest lines to vanish.

It is well known that in the solar spectrum many lines of metals known to be in the Sun's atmosphere are absent. Thus only two aluminium lines are present, while only two nickel lines are absent; and on comparing the solar spectrum with maps of the lengths of the elemental lines, it was found that, without exception, the lines reversed are the longest lines. Zinc, aluminium, and (possibly) strontium were thus added to the list of solar metals given by Thalén, who rejected zinc from Kirchhoff's list, and agreed with him in rejecting aluminium.

It would appear that a study of Sun-spot phenomena by the light of this knowledge will conduce considerably to our knowledge of the solar cyclical changes, and especially of those which accompany the maximum and minimum spot periods.

The full results of this inquiry were communicated to the Royal Society in December 1872.

The research was continued by an investigation of the character of compound spectra, as distinguished from truly elementary ones; and it was shown with regard to the salts of strontium that each of them had a distinct spectrum peculiar to itself, when proper precautions were taken to isolate from disturbing influences, and not to heat it to such a degree as to cause extensive dissociation, whilst when heated in the air by the spark or in a flame these bodies gave the spectrum of strontic oxide. The distinctive feature of all these compound spectra is, that they are made up of *bands*, or channelled spaces, instead of lines, which are the characteristic of truly elemental bodies.

The researches of Secchi and Rutherfurd have shown that there are classes of stars distinguished: (1) by great brightness and few lines; (2) by medium brightness and many lines; (3) by little brightness and the presence of bands to the exclusion of lines.

The latter spectra prove beyond doubt the presence of compound vapours in the atmospheres of these stars, and hence in these experiments we may have the exact equivalents of the phenomena of variable stars; phenomena due possibly to a delicate state of heat equilibrium, the disturbance of which caused the star to give now the band absorption of compound molecules, now the line absorption of elementary atoms.

The series of salts studied for the purpose of ascertaining the existence of compound spectra consisted of three members of the haloid group of strontium salts, $Sr\,Cl_2$, $Sr\,Br_2$, $Sr\,I_2$. It was found that the fourth, $Sr\,F_2$, was, in common with $Ba\,F_2$ and $Ca\,F_2$, so refractory as to refuse to give a spectrum; and it was observed that the heat required to render the spectrum of a compound body visible dissociates it according to its volatility, a fact which is peculiarly manifest in the group of monad metals, including sodium, all of which are distinguished for their volatility and highly electro-positive character.

The result as to each compound possessing, when not dissociated, a peculiar spectrum, confirms the results obtained by Mitscherlich, and by Clifton and Roscoe, while it was shown that, as suggested by former observers, the spectra obtained by Bunsen and Kirchhoff in the Bunsen-flame from the oxysalts and haloid salts of barium, strontium and calcium, were really due to the oxides of those metals, though ascribed by the illustrious authors to the metals themselves.

The results of experiments on chlorides, bromides, and iodides having shown that, when subjected to a heat of dissociation the lines of the metal shortened, as a rule, as the atomic weight of the metalloid increased.

Experiments were made with mechanical mixtures, alloys being taken for the purpose. It was found that the loss of the lines of one constituent was a measure of its amount in any given alloy, the lines being greater in number as the metal to which they were due was greater in percentage in the alloy under examination.

Hence it would seem that the number of reversed lines, in the case of any metal in the Sun, is a measure of its amount in the reversing layer. But here the effect of pressure comes in; for, as pressure increases the number of lines given by a metal, a number of reversed lines which would represent 90 per cent. under terrestrial barometric pressures would not do so in an atmosphere the pressure of which was much greater.

It will follow from this that we shall know the pressure of the solar atmosphere when we find that at which the percentages represented by the reversed lines of the whole of the solar metals shall, when added together, exactly amount to 100.

In order further to develop this branch of the research the

electric arc has been employed, and a new method of photographing spectra has been introduced.

A camera carrying a 5 × 5 inch plate and a 3-inch lens of 23 inches focus replaced the observing telescope of the spectroscope. The lens focused from W.L. 3,900 to W.L. 4,500 very fairly upon the plate. The beam passing through collimator and prisms was, as in Mr. Rutherfurd's researches, very small. As the electric arc in its usual vertical position gave all the lines from pole to pole, the lamp was placed on its side, and the arc used in a horizontal position, the slit being vertical. The dense core of the arc then gave all the short lines in the centre of the field, the longer ones extending beyond them on either side. In order to obtain a scale, it was resolved to photograph the solar spectrum immediately adjacent to the metallic spectrum under examination.

To effect this a portion of the slit was covered up while the solar spectrum passed through the free part, and then the part used for the solar spectrum was covered, whilst the formerly covered part was opened for the metallic spectrum. This was effected by a shutter, with an opening sliding in front of the slit.

The image of the Sun was brought to a focus between the poles of the lamp by an extra lens interposed between the lamp and the heliostat.

The use of the shutter permits the comparison of either two or more spectra upon a single plate, or the solar spectrum may be compared with two metallic spectra, being made to occupy the position between the two.

The examination of the various spectra of metals and alloys indicated the great impurity of most of the metals used, and suggested the possibility of the coincidences observed by Thalén, and others being explained in the light of the former work.

It is observed that coincidences are particularly numerous in the spectra of iron, titanium, and calcium, and that nearly every other solar metallic spectrum has one or more lines coincident with lines of the last element. These coincident lines are, as a rule, very variable in length and intensity in various specimens of the metals in which they occur, and are sometimes altogether absent.

The longest lines of calcium occur in iron, cobalt, nickel, barium, strontium, &c., and the longest lines of iron occur in calcium, strontium, barium, and other metals.

The following general statements are hazarded, with a premise that further inquiry may modify them:—

1. If the coincident lines of the metals be considered, those cases are rare in which the lines are of the first order of length in all the spectra to which they are common; those cases are much more frequent in which they are long in one spectrum and shorter in the others.

2. As a rule, in the instances of those lines of iron, cobalt,

nickel, chromium, and manganese which are coincident with lines of calcium, the calcium lines are long, while the lines as they appear in the spectra of the other metals are shorter than the longest lines of those metals. Hence we are justified in assuming that short lines of iron, cobalt, nickel, chromium, and manganese, coincident with long and strong lines of calcium, are really due to traces of the latter metal occurring in the former as an impurity.

3. In cases of coincidence of lines found in various spectra the line may be fairly assumed to belong to that one in which it is longest and brightest.

The previous researches having shown that the former test for the presence or absence of a metal in the Sun—namely, the presence or absence of its brightest or strongest lines in the average solar spectrum—was not conclusive, a preliminary search for other metals was determined on; and as a guide, two lists were prepared, showing broadly the chief chemical characteristics of the elements traced and not traced in the Sun.

The Tables showed that in the main those metals which had been traced formed stable compounds with oxygen.

Mr. Lockyer therefore determined to search for the metals which formed strong oxides, but which had not yet been traced.

The result up to the present time has been that strontium, cadmium, lead, cerium, uranium, and potassium would seem with considerable probability to exist in the solar reversing layer. Should the presence of cerium and uranium be subsequently confirmed, the whole of the iron group of metals will thus have been found in the Sun.

Certain metals forming unstable oxides, such as gold, silver, mercury, &c., were sought for and not found. The same was the case when chlorine, bromine, iodine, were sought by means of their lines produced in tubes by the jar spark. These elements are distinguishable as a group by forming compounds with hydrogen.

It was observed that certain elementary and compound gases effect their principal absorption on the most refrangible part of the spectrum when they are rare, and that as they become dense the absorption approaches the less refrangible end—that the spectra of compounds are banded or columnar, the bands or columns lying at the red end of the spectrum—that the spectra of chlorine, bromine, iodine, &c. are columnar, and that these are broken up by the spark, just as the band spectra of compounds are broken up; and that it is probable that no compounds exist in the Sun. The following facts are gathered from the work already accomplished by Rutherfurd and Secchi, namely:

That there are three classes of stars:—

1. Those like *Sirius*, the brightest (and therefore hottest?) star in the northern sky, their spectra showing only hydrogen lines very thick, and metallic lines exceedingly thin.

2. A class of stars with a spectrum differing only in degree from those of the class of *Sirius*, and to this our Sun belongs.

3. A class of stars with columnar or banded spectra, indicating the formation of compounds.

The question arises whether all the above facts cannot be grouped together in a working hypothesis, which assumes that in the reversing layers of the Sun and stars various degrees of "celestial dissociation" are at work, which prevents the coming together of the atoms which, at the temperature of the earth, and at all artificial temperatures yet attained here, form the metals, the metalloids, and compounds.

In other words, the metalloids are regarded as *quasi* compound bodies when in the state in which we know them; and it is supposed that in the Sun the temperature is too great to permit them to exist in that state in the reversing layer, though they may be found at the outer portions of the chromosphere or in the corona.

It is suggested that if this hypothesis should gain strength from subsequent work, stony meteorites will represent the third class of metalloidal or compound stars, and iron meteorites the other, or metallic stars.

The paper communicated to the Royal Society concludes as follows:—

"An interesting physical speculation connected with this working hypothesis is the effect on the period of duration of a star's heat which would be brought about by assuming that the original atoms of which a star is composed are possessed of the increased potential energy of combination which this hypothesis endows them with. From the earliest phase of a star's life the dissipation of energy would, as it were, bring into play a new supply of heat, and so prolong the star's life.

"May it not also be, if chemists take up this question, which has arisen from the spectroscopic evidence of what I have before termed the plasticity of the molecules of the metalloids taken as a whole, that much of the power of variation which is at present accorded to metals may be traced home to the metalloids? I need only refer to the fact that, so far as I can learn, all so-called changes of atomicity take place when metalloids are involved, and not when the metals alone are in question.

"As instances of these, I may refer to the triatomic combinations formed with chlorine, oxygen, sulphur, &c., in the case of tetrad or hexad metals. May not this be explained by the plasticity of the metalloids in question?

"May we not from these ideas be justified in defining a metal, provisionally, as a substance the absorption spectrum of which is generally the same as the radiation spectrum, while the metalloids are substances the absorption spectrum of which, generally, is not the same?

"In other words, in passing from a hot to a comparatively

cold state, the plasticity of these latter comes into play, and we get a new molecular arrangement. Hence, are we not justified in asking whether the change from oxygen to ozone is but a type of what takes place in all metalloids?"

Recently the photographic method used by Mr. Lockyer has been applied by him to a study of certain metallic meteorites, and a comparison of their spectra with those of Fe, Ni, Co, Mn, Al, Mg, &c. This research is at present very incomplete, but enough is known to warrant the belief that these metallic meteorites are of a much more complicated chemical composition than has hitherto been supposed.

The Earl of Rosse's Observatory.

Reference is made in another section of the Report to the results of the experiments made at this Observatory on Lunar Radiation, a work which has occupied the attention of Lord Rosse since the year 1868. The details of the observations were laid before the Royal Society in March 1873, and formed the subject of the Bakerian Lecture. In the meantime the observations of the Nebulæ, contained in Sir John Herschel's general catalogue, have not been discontinued; but since the completion of the experiments on Lunar Radiation left us more leisure, they have been carried on with more assiduity. Owing to the application of a fairly accurate clock-movement to the 6-foot telescope, micrometrical measures are now made with more ease and accuracy than before, and therefore observations of this class are more numerous than in earlier years; but most of the brighter and more interesting class of objects having been examined already, there is now less scope for the use of the pencil.

Jupiter was frequently observed during last spring, and careful coloured drawings made on many occasions. *Uranus* and *Neptune* were also examined as far as was practicable, and a few other miscellaneous observations were made.

Royal Observatory, Cape of Good Hope.

Since the last report, the 'Cape Catalogue of 1159 Stars for the Epoch 1860,' deduced from observations made at this Observatory during the years 1856—1861, has been completed and published.

The observations of the stars in Lacaille's Catalogue in the zone 165° N.P.D. to 175° N.P.D., comprising almost all stars down to the seventh magnitude, have been completed, at least

three observations of each star having been made; and the reductions are in an advanced stage.

A working list has been prepared for the zone 145° N.P.D. to 165° N.P.D., and observations of these stars were commenced at the beginning of the year.

At the request of Dr. Galle, observations of *Flora* at opposition were undertaken; but, owing to unusually bad weather, observations could only be made on fourteen nights.

The reductions of the observations made in the years 1859 and 1860 have been completed; and the results printed, with the exception of some of the Planetary work and the Introductions.

Mr. Stone regrets to have to announce the death of Mr. W. Mann, for many years First Assistant at this Observatory. He has been succeeded by Mr. W. H. Finlay, B.A. Since the last report, Mr. C. M. Stevens has also been appointed on the staff of the Observatory, as Third Assistant.

Melbourne Observatory.

During the past year very little change has been made either in the instruments or ordinary routine work of the Observatory. The catalogue of 1,227 stars for the epoch 1870, being the result of the Transit Circle observations from 1863·5 to 1871·0, was completed at the beginning of 1873, and has been for some time ready for the printer. The Transit Circle has been fully employed during 1873 in observing, besides the ordinary clock stars, &c., a catalogue of stars which pass the meridian near the northern horizon, and of stars situated between 144° and 180° of N.P.D., for the special object of obtaining the amount of refraction on both sides of the zenith. The standard circumpolar stars were also well observed during the winter, which proved exceptionally favourable for the work, so that scarcely any sets had to be rejected through failure of getting the corresponding sets, twelve hours afterwards.

Volume IV. of the *Melbourne Observations*, containing the results of the transit work for 1869 and 1870, has been published, and is now in process of distribution.

No zone observations have been made during the past year, but it is intended to resume them soon. The telescope of 6¼ inches aperture, which has been employed on this work, has been mounted as a finder to the large reflector, and used as an auxiliary to the observations of *Flora* during the opposition of that planet.

The large reflector has been in almost constant use. Several very excellent photographs of the Moon have been obtained, a liberal distribution of which has been made to the schools and Mechanics' Institutes of the Colonies, and to the different

scientific bodies. Several drawings of the nebulæ have also been made, the result of which still confirms the great changes to which these bodies are subject. This instrument has been employed in observing *Flora*, and the comparison stars selected by Dr. Galle, as a test of the Sun's parallax. The drawings that have been made by the different observers with this telescope are at present in the hands of the lithographers, and it is hoped that they will soon be ready for publication. The large mirror A is still in use; it has had its polish restored by a cleaning with washed chalk and alcohol; the small mirror has been re-figured as well as re-polished, and the improvement in definition caused by these alterations is very decided.

The 12-inch reflector has not been used for any astronomical work, but it has proved of great service in furnishing the means of acquiring experience in the grinding and polishing of metallic specula.

A change has been made in the method of sending the time-ball signals along the telegraph lines. For some time it was a matter of complaint that the signals made by the toothed wheel and contact springs were too short in duration, thus necessitating a very delicate adjustment to get them at all; this has been remedied by a contact made by a pin fixed to the pendulum at about 10 inches from the top, which at each oscillation passes through a cup of mercury. By this means the duration of the contact can be varied at pleasure, and since its adoption all complaints have ceased.

The magnetical and meteorological observations have been pursued as usual; a new self-registering rain-gauge has been added to the establishment, and the other instruments are all in good order. A monthly abstract has been published of the work in this department, and this will be supplemented each year by an annual record.

The Government of Victoria have liberally voted a sum of 1,300*l.* for finding the necessary instruments, &c. for observing the forthcoming Transit of *Venus* across the Sun's disk. In addition to the principal station at Melbourne, it is intended to equip two other stations in the colony; they are not yet selected, but the principal considerations in their choice will be accessibility and probability of cloudless weather.

Sydney Observatory.

Since the date of the last report satisfactory progress has been made with the regular work of the Observatory, both astronomical and meteorological. The equatoreal was thoroughly repaired and put in order, and is now in as good condition as when first received. A new barograph and tide-gauge

have been made, and are now in operation; and a 10-inch reflecting telescope mounted. Two reflectors of colonial glass, 10¾ inches diameter, have been constructed at the Observatory, and the results are so satisfactory that Mr. Russell hopes to construct a large one, so that the work for which they are specially suited may be taken up. A part of this, the register of coloured stars, has already been commenced.

A new driving clock for the equatoreal has also been made, the governor of which is a short open cylinder, 1¾ inch outside diameter, and 1½ inch inside. This works in a vertical axis, with the open end in mercury, and it is so arranged that any increase of power in the working parts forces the cylinder into the mercury and increases the friction in proportion to the increased weight, so that the weight may be doubled without altering the speed, or it may be so arranged that an increase of weight will make it go slower; variations in the friction of the instrument are therefore under the complete control of the clock.

The ordinary observing-work during the year preceding the date of Mr. Russell's report included 1,488 transits, with the usual instrumental adjustments; about 370 measures of the distance and position-angle of double stars; 155 measures in R.A. of cluster stars, and 80 of declination; and numerous positions of yellow, blue, and red stars.

During the year 1873 the work of the Observatory included the regular astronomical observations; the determination of the positions of as many towns as possible; the carrying on and the improvement of the regular meteorological work at Sydney and country stations, with the daily and monthly publication of results; the usual time signals; the preparation of instruments and observers for the Transit of *Venus*; and making some observations with a powerful telescope on the Blue Mountains, to ascertain the advantage to be gained from an elevation from 2,000 to 4,000 feet.

The Government of New South Wales have granted 1,000*l.* towards the expenses of the approaching Transit of *Venus*.

Notes on some Points connected with the Progress of Astronomy during the Past Year.

Discovery of Minor Planets.

The following five minor planets have been discovered since the last anniversary:—

(130) *Electra*, discovered at the Litchfield Observatory, Clinton, New York, by Dr. C. H. F. Peters, on February 17, 1873.

(131) *Vala*, discovered on May 24, also by Dr. Peters, at Clinton.

(132) *Æthra*, discovered by Mr. J. Watson, on June 13, at the Ann Arbor Observatory, Michigan, U.S.

(133) *Cyrene*, discovered at Ann Arbor, by Mr. Watson, on August 16. On July 29 Mr. Watson observed an object of the 12th magnitude, which he soon identified as a planet, its position at $14^h\ 1^m\ 0^s$ mean time being R.A., $23^h\ 8^m\ 12^s{\cdot}35$, and N.P.D. 92° 23′ 11″·0; but on account of cloudy weather and bright moonlight, he had no opportunity of re-observing it before the night of August 16, when several observations were made of an object which Mr. Watson considered at the time to be the new planet of July 29. From the observations of August 16, 17, and 23, elements of its orbit were computed, which represented well the observed places of the planet to September 2; but for July 29 they gave the R.A. $23^h\ 12^m\ 41^s{\cdot}2$, and the N.P.D. 92° 23′ 42″, clearly showing that the planet discovered on August 16 was a different object from that observed on July 29. Mr. Watson made a search for the missing planet whenever the weather permitted, but without success. This object must therefore still be classed among the undiscovered planets.

(134) *Sophrosyne*, discovered by Dr. R. Luther, at Bilk, on September 27, 1873.

Since the last anniversary, the following minor planets have been named :—(119) *Althæa*, (121) *Hermione*, (125) *Liberatrix*, (126) *Velleda*, (127) *Johanna*, (128) *Nemesis*.

Discovery of Comets.

Seven comets have appeared during the last twelve months; three, and probably four, of them being periodical comets which have returned to perihelion, and were therefore expected. The dates of their first observation are as follows :—

Comet I. 1873 (Tempel's periodical comet of 1867), was first observed by M. Stéphan, at Marseilles, on April 3, 1873.

Comet II. 1873, discovered by M. Tempel, at Milan, on July 3.

Comet III. 1873, discovered by M. Borrelly, at Marseilles, on August 20.

Comet IV. 1873, discovered by M. Paul-Henry, at the Observatory of Paris, on August 23.

Comet V. 1873 (Brorsen's periodical comet), was first observed at Marseilles, by M. Stéphan, on September 20.

Comet VI. 1873 (Faye's periodical comet), was detected at Marseilles, by M. Stéphan, on September 3.

Comet VII. 1873, discovered by M. Coggia, at Marseilles, on November 10; and also independently by Dr. Winnecke, at Stras-

burg, on November 11. Mr. Hind and Dr. Weiss have both remarked that the elements of this comet bear a great resemblance to those of Pons' first comet of 1818.

Telegraphic Announcements of Astronomical Discoveries.

During the past year a very important arrangement has been concluded, by which is guaranteed the free transmission of cable telegrams between Europe and America of accounts of astronomical discoveries, which, for the purpose of co-operative observations, require immediate announcement. In a circular issued by the Smithsonian Institution, Dr. Henry thus explains the importance of the concession so liberally made on the part of the Telegraph Companies:—"Among such discoveries are those of planets and comets, or of bodies which are generally so faint as not to be seen, except through the telescope; and which being in motion, their place in the heavens must be made known to the distant observer, before they so far change their position as not to be readily found. For this purpose the ordinary mail conveyance, requiring at least ten days, is too slow, since in that time the body will have so far changed its position as not to be found, except with great difficulty; and this change will become the greater if the body is a very faint one, for in that case it could only be discovered on a night free from moonlight, which of necessity in ten or twelve days must be followed by nights on which the sky is illuminated by the Moon, and all attempts to discover the object would have to be postponed until the recurrence of a dark night. Indeed, even then the search often proves in vain; and it is not, in some cases, until after a set of approximate elements are calculated and transmitted, that the astronomers on the two sides of the Atlantic are able fully to co-operate with each other." These difficulties having been discussed by some of the principal astronomers in Europe and America, the subject was laid before the Committee of the Smithsonian Institution by Dr. C. H. F. Peters, of Hamilton College, Clinton, U.S. The Institution immediately applied to the Directors of the Associated Companies, who at once granted the free use of all their lines for the object in question, both from Europe and America, for a limited number of telegrams during each year.

Although the discovery of planets and comets will probably be the principal subject of the cable telegrams, yet it is not intended to restrict the transmission of intelligence solely to that class of observation. Any remarkable solar phenomenon presenting itself suddenly in Europe, observations of which may be practicable in America several hours after the Sun has set to the European observer; the sudden outburst of some variable star similar to that which appeared in *Corona Borealis* in 1866;

unexpected showers of shooting-stars, &c., would be proper subjects for transmission by cable. The great value of this concession on the part of the Atlantic Telegraph and other companies cannot be too highly prized, and our science must certainly be the gainer by this disinterested act of liberality. Already planets discovered in America have been observed in Europe on the evening following the receipt of the telegram, or within two or three days of their discovery.

The Council are glad to be able to announce that the Director of the Imperial Russian Telegraph has also given permission for the free transmission of messages relating to new astronomical discoveries within the boundaries of the Russian Empire.

Preparations for Observing the Transit of Venus.

At the November meeting the Astronomer Royal gave an oral account of the state of the British preparations for the approaching Transit of *Venus*. The first point to be noticed was the selection of stations, due regard being had to what other Governments might be expected to do. Originally five stations were chosen viz.: Alexandria, Honolulu, Rodriguez, New Zealand, and Kerguelen Island, at which the ingress or egress would be greatly accelerated or retarded, the observations then contemplated consisting entirely in noting the time of contact of *Venus* with the Sun's limb.

But it was subsequently proposed by Mr. De La Rue to employ photography to depict *Venus* while on the Sun's disk, and it resulted from this and also from some considerations founded on what Mr. Proctor has pointed out, that a photoheliograph might be advantageously established in the North of India. The Indian Government acceded to the Astronomer Royal's proposal, and subsequently (as announced by Colonel Strange at the January meeting) decided to supplement the photoheliograph with telescopes for eye observation and with the instruments required for determination of local time and geographical position, the whole being under the superintendence of Colonel Tennant. This is the only *new* addition to the original selection of stations; the other changes made have been introduced with the view of strengthening some of the original stations.

From various circumstances it seems probable that the French Government will not be in a position to occupy the station at the Marquesas Islands, on which the Astronomer Royal relied for strengthening the very important station at Honolulu, so that it becomes desirable to supply such aid from our own resources, by establishing two supplementary stations, one at Hawaii, which is quite at the eastern extremity, and another at Atooi, at the western extremity of the group. By thus distributing three stations over a distance of 200 or 300 miles, the

chances of fine weather will be greatly increased. An additional station is also to be established in the south of Kerguelen Island, the principal station being at Christmas Harbour, in the north of the island. The idea of sending an expedition to the Macdonald Islands has now been abandoned, as it is considered that the observations there will be sufficiently provided for by the American and German parties, who will, it is to be hoped, be able to effect a landing there with their instruments.

The general principle in the selection of these stations has been, that no party of astronomers should be sent to a station where there were no human beings, and where a boat could not land once in a month. This consideration at once excludes many stations which are, from a mere geometrical point of view, favourably situated, for both the Astronomer Royal and the chief officers of the Admiralty have decided that they will have nothing to do with a place where there is neither anchorage nor human beings, though they are willing to dispense with one of these conditions, as at Rodriguez, where there is no anchorage; and at Kerguelen Island, where there are no inhabitants. The same practical view has been taken by other nations in their choice of stations. The Americans have selected eight stations, the Germans four, the French five, the Russians twenty-seven,* and the English nine, besides Lord Lindsay's station at the Mauritius, from which a most valuable series of observations of all kinds is to be expected.

At the English stations the observers will be, for the most part, naval officers, with the addition of some officers of the Engineers and Artillery, and some private observers, who have been, for some time past, undergoing a systematic course of instruction in the use of all the instruments, and in the taking of all the observations necessary, not only for the actual transit but also for the determination of the exact longitude of the observer's station. Each observer has also been instructed in photography, for when groups of a small number of persons are sent to outlandish stations, every one must have a knowledge of each branch of the service to which he is attached. All this involves great labour on all sides.

The instruments which have been provided are: for each of the five principal stations, a 6-inch equatoreal, a 4-inch telescope, a photoheliograph, a transit, and an altazimuth; for the subsidiary stations, a 4-inch telescope and a portable transit for determining local time; the longitude being left to be determined at some other time, if the observations should have been successful.

A very important feature in the arrangements has been the preparation of a working model representing the circumstances of the Transit of *Venus* with great exactness, and enabling the

* A list of the Russian stations, communicated by M. Struve, is inserted in the *Monthly Notices*, vol. xxxiii. pp. 415, 416.

observers to familiarise themselves with the various aspects which the 'black-drop' presents under different circumstances. In this model a black disk is carried by clock-work across an opening in a plate of metal, through which direct sunlight is reflected by a mirror to an observer at a telescope 400 feet distant. The opening in the plate represents the Sun's disk, the edges having the proper curvature of the Sun's limb, and the black disk represents exactly the planet *Venus*, having the same apparent diameter and angular motion. The discordances between different observers and different telescopes can thus be determined, and further, the effect of cloud or haze may be allowed for. By using alternately full sunlight and ordinary cloudlight, the model clock being stopped, the black ligament may be seen to appear and disappear, though by means of dark glasses the light which reaches the eye is reduced to the same intensity in the two cases. With full sunlight the ligament is still visible eight or ten seconds after true contact, but with dull light it disappears at the third second after contact; it is therefore to be expected that any atmospheric cause which diminishes the Sun's brightness will affect the formation of the ligament, by diminishing the contrast between the light of the Sun and the blackness of *Venus*. The model has also been used with great success for measuring cusps and distances of limbs with the Astronomer Royal's double-image micrometer, and much valuable information has been gained thereby.

The photographic part of the preparations has required much care and continual attention, and here the assistance of Mr. De La Rue has been of the greatest service. The image of the Sun formed at the principal focus of the photoheliograph (which is a greatly improved form of the Kew instrument) is about half an inch; this is enlarged by the secondary magnifier (Dallmeyer's rapid rectilinear doublet) to 4 inches, which is therefore the diameter of the picture of the Sun, whilst that of *Venus* (nearly equal to that of the Earth) is 1-30th part of 4 inches, and the results at different stations will consequently differ by a fraction of this 30th part of 4 inches. It is, then, of the utmost importance to get a well-defined limb to measure from, and this must be the guide in the selection of the photographic process to be adopted. The pyrogallic development, used with such success by Mr. De La Rue for delineating the Sun's disk, does not appear to give the necessary sharpness of limb, which is, however, obtained under proper conditions with the ordinary iron developer, though the deposit of silver is in this case rather coarser. It appears, however, that the albumen dry plate process (as perfected by Capt. Abney, R.E.) combines the advantages of both methods, giving a fine deposit and a sharp limb, free from halation, with a moderate exposure; and it has this further merit, that fewer operators are required. Accordingly both Russia and Germany have decided to adopt it as well as England, so that all the photographs will probably be taken by the same process, and will therefore be strictly comparable, whilst one possible

source of error, shrinkage of the collodion film, will be entirely eliminated.

It remains to notice the plan proposed by M. Janssen for securing photographs of the actual contact, by taking a number of pictures on one plate at intervals of a second or two, each picture including the planet *Venus* and the adjacent part of the Sun's limb only. Some experiments have been tried in which pictures were taken every five seconds; and from these it appears that the practical difficulty in carrying out this proposal is to get a sufficiently rapid exposure without communicating a tremor to the telescope; but the importance of obtaining representations of the black drop in various stages, for comparison with corresponding eye observations, is so great, that no pains will be spared to overcome this difficulty.

In India, Colonel Tennant, will, if possible, be provided, in addition to the photoheliograph, with a 6-inch telescope, the new great theodolite, a transit-instrument, a chronograph, and clocks. The clocks he proposes to control by a method which has the following properties: The control is made at the middle of the vibration, both of regulator and governed clocks, and the *same* current works the chronograph, so that there is no difference between clocks and chronograph; and if the latter fail, observations by eye and ear can be used without any uncertain correction to the errors. The photoheliograph is now nearly completed, under the direction of Mr. De La Rue, and the remaining instruments are in course of construction under the supervision of Colonel Strange.

Colonel Tennant also proposes to send an officer, with a 6-inch telescope, as far west as there is a reasonable chance of fine weather, to make eye observations of the last contact. He will have a transit instrument for time, and Colonel Tennant has found one available which he expects will answer his purpose. This supplementary station will most probably meet with the approval of the Indian Government, who have hitherto liberally sanctioned all the expenditure which has been asked for.

A station near Roorkee has been connected with the Great Trigonometrical Survey by Colonel Walker's orders, and it will be easy to get the co-ordinates of Colonel Tennant's station. It is also proposed to determine these astronomically, because the attraction of the Himalaya mountains must be very sensible there. This only involves a difference of longitude from Madras, which is expected not to be very laborious.

The second photoheliograph ordered by Mr. De La Rue for the Russian Government was completed by Mr. Dallmeyer last year, and was despatched before the navigation had been stopped by frost. News of its safe arrival has been received. Of the three remaining photoheliographs which are being constructed by Mr. Dallmeyer under Mr. De La Rue's direction, one, namely, that for India, is nearly completed, and will be sent to Colonel Tennant in the course of this month; the second, for the Government of Victoria, which has been only recently ordered, will, it is

hoped, be ready for shipment to Mr. Ellery by the end of May; and the third, for Mr. Meldrum, of the Mauritius Observatory, will probably be ready to be forwarded in September next.

Atlas Cœlestis Novus of Dr. Heis.

Everyone accustomed to the formation of star-catalogues have been much indebted to our Associate, Professor Argelander, for his *Uranometria Nova*, which contains the observed magnitudes on a uniform basis of all the stars visible to his unassisted eye. These magnitudes have universally been adopted as standard values. The necessary labour required to perform so elaborate an examination is very great, but this has not prevented another zealous German astronomer, Dr. Edward Heis, of Münster, from following the example of Prof. Argelander in producing a most valuable and original work on stellar magnitudes, after continuous observation during twenty-seven years. The Catalogue and Atlas, entitled *Atlas Cœlestis Novus*, which Dr. Heis has recently published, may be considered as a continuation of Argelander's *Uranometria Nova*, as it contains a considerable number of additional stars which the keen-sightedness of Dr. Heis has enabled him to see with the naked eye, and which were passed over by his illustrious predecessor. The number of stars observed by Dr. Heis, whose magnitudes are recorded in his catalogue, are 5,421, being 2,153 in excess of those observed by Prof. Argelander. It may be remarked that the variable star 37 *Capricorni* was seen in 1852 and 1865 with the naked eye, but in 1863 and 1870 it was invisible. Dr. Heis' magnitudes are all determined from his own estimations, for his general plan of operations consisted in comparing the relative magnitudes of the stars as viewed by the naked eye, without having regard to what had already been published. The varying brightness of the *Milky Way* is excellently represented in the Atlas in different shades, which gives a very good effect, but this is somewhat diminished by the insertion of the figures of the constellations. The independent estimations of Argelander and Heis agree generally; but the large number of additional stars, especially of the 6½ magnitude, makes the catalogue of the latter more than usually valuable. The excess of Heis over Argelander is:

4	Magnitude	1
5·4	"	3
5	"	8
5·6	"	2
6·5	"	20
6	"	292
6·7	"	1821
Clusters	"	3
Nebulæ	"	3

The arrangement of the catalogue is similar to Argelander's *Uranometria Nova*. It would, however, have been more convenient for reference if the approximate right ascension had been given in hours instead of in degrees, as practically that element is now scarcely ever referred to in arc; and more especially because, in the formation of a standard star-catalogue, no one would at the present time think of giving the right ascension of the stars otherwise than in time.

Washington Catalogue of 10,658 *Stars.*

A very extensive Catalogue of Stars, compiled from observations made at the United States Naval Observatory, at Washington, during the years 1845 to 1871, has been recently published. It has been arranged for publication by Professor Yarnall, of that Observatory, and is a valuable contribution to observing astronomy. The catalogue contains 10,658 stars which had been observed for miscellaneous purposes with the transit instrument, mural-circle, meridian-circle, and prime-vertical transit instrument. It includes stars observed in the army and coast surveys, and also many from Lacaille's catalogue, which had not hitherto been re-observed. Most of them are above the 9th magnitude, and nearly all the smaller stars in the catalogue were used as comparison-stars by Mr. Ferguson in his observations of the minor planets with the equatoreal. As the observations incorporated in the catalogue extended over a period of twenty-seven years, they had been originally reduced with very different data; and the most important preliminary step taken by Professor Yarnall was, to reduce the various data employed to one uniform standard. A few of the places depend upon a single observation in each element; and occasionally, but not frequently, the place is given for one element only. Most of the stars have, however, been observed two or more times, while the places of some of the principal stars, which have been used to determine the clock-error, depend upon a very large number of observations. The epoch of the catalogue is 1860·0.

The catalogue gives the mean right ascension and declination of each star, together with its magnitude, the mean year of observation, the number of observations, and the annual precession for 1860. For ordinary approximate calculation of stars' places these data are sufficient, but in original and fundamental catalogues of stars the addition of columns containing the secular variation and proper motion, when known, for each star is of great assistance to the computer. In the *Washington Catalogue* these additional columns could have been given without sensibly increasing the bulk of the volume. The value of this excellent catalogue would also be considerably increased if the separate star-constants in both elements, for reduction to apparent place, had been included in the work, either with reference to Airy's or Bessel's day numbers given in the *Nautical Almanac*.

In addition to the stars observed with the ordinary instruments, the right ascension and declination of the components of the *Pleiades* and *Præsepe* are included in the text. The group of *Pleiades* was observed by Mr. Ferguson with the equatoreal, and that of *Præsepe* by Professor Hall, with the same instrument. Many of these stars, however, have also been observed with the transit-instrument and mural-circle, the results being given separately from those observed with the equatoreal, but in juxtaposition, or in some cases the meridional observations alone are inserted. This valuable work will be found to be a most useful addition to the library of the computing-rooms of our observatories.

The Cape Catalogue of 1,159 *Stars.*

The first standard catalogue of stars issued from the Cape Observatory has lately been published, under the superintendence of Mr. Stone. It contains the mean results of the right ascensions and north polar distances of stars observed with the Transit-Circle, under the direction of Sir Thomas Maclear, in the years 1856 to 1861, the former year being that adopted by Mr. Stone for the commencement of the reductions. The present catalogue has been formed from the results of observations contained in the volumes for the years 1856 and 1860, already published, with the addition of the observations of stars near the South Pole in 1861. The epoch of the catalogue is 1860, which is considered by Mr. Stone to afford facilities for a comparison between the Greenwich and Cape results, and also for the formation of a more general catalogue for the whole of the heavens, by the combination of the principal northern and southern catalogues reduced to that epoch.

The Cape Catalogue gives, in addition to the mean R.A. and N.P.D. for 1860, the precession, secular variation, and annual proper motion; the corresponding Nos. for reference in the Catalogues of Lacaille, Brisbane, Fallows, Johnson, Henderson, the Greenwich 7-year (1860), and, occasionally, the Greenwich 7-year (1864) are also inserted. In a few cases, reference is also made to the Radcliffe Catalogue for 1860.

Harvard Astronomical Photographs and Drawings.

A beautiful collection of pictorial representations of astronomical phenomena has been issued from the Observatory of Harvard College, Cambridge, U.S., to which the Council desire particularly to invite attention. The different subjects are delineated with considerable accuracy as to the details, and the complete series forms a valuable set of trustworthy illustrations of popular

celestial objects. Among them are copies of photographs of the Sun, showing groups of solar spots, and faculæ in visible connection with solar prominences; drawings of successive appearances of a remarkable Sun-spot visible from March 28 to April 4, 1872; of solar prominences of considerable magnitude observed in 1872; of the planets *Jupiter* and *Saturn*; of the lunar craters *Torricelli*, *Agrippa*, *Godin*, *Eratosthenes*, *Hypatia*, *Guttemberg*, and *Julius Cæsar*; and copies of photographs of the total eclipses of the Sun, taken at Shelbyville, Kentucky, on August 17, 1869, and at Jerez de la Frontera, Spain, on December 22, 1870. All the drawings have been carefully lithographed, and some of the details are shown very clearly, especially the lights and shadows of the lunar craters, the natural appearance of the lunar surface being exhibited in a very satisfactory manner.

Schmidt's Map of the Moon.

This remarkable representation of the lunar surface, which has occupied the attention of Professor Schmidt during thirty-four years, is now completed. Those who have seen this magnificent work speak of it in the highest terms, not only of its general appearance, but also of the wonderful manner with which the details of the lunar features are delineated. The map is two mètres in diameter, and the drawing is made with the most extraordinary care and precision, the minuteness of the work being almost beyond conception. A specimen of the map has been exhibited at one of the meetings of the Society, when it was much admired for the extreme delicacy with which all the details of the lunar surface are given, and a hope was generally expressed that the map in its entirety would eventually be published.

Unfortunately, the funds of the Athens Observatory are too limited to permit the publication of this great work at the expense of that establishment, and Prof. Schmidt fears that there is no chance of publishing it either in Greece, France, or Germany, owing to the expense, which must necessarily be great. It is hoped, however, that some means will be provided for engraving this beautiful production, and thus make it available to astronomers. Meanwhile, Professor Schmidt has been requested to forward an estimate of the probable expense of transferring the map upon stone for lithographic engraving. If the estimate be not excessive, probably some means may be found to preserve to our science the valuable results of Professor Schmidt's thirty-four years' labour.

Zone Observations at the Cordoba Observatory.

Dr. Gould having essentially completed his estimations of the magnitudes of the stars visible to the naked eye in the southern

hemisphere, has now commenced in earnest the observations of the zones of the stars, which was the primary object of the establishment of the Cordoba Observatory. Whenever the weather permitted, Dr. Gould and his assistants have been enabled to observe three zones nightly. Four stars were observed both before and after the zone observations for the determination of the instrumental corrections, the entire series of observations occupying at least eight hours each night. Dr. Gould thus writes: "During the intervals between the zones I give rest to my eyes, the assistant who has read the microscopes for the first zone taking the observations for instrumental determinations; and being himself relieved at the microscopes by a colleague, who also officiates during the second zone while the former enjoys a period of rest. Thus the routine gives alternately to each recorder at the microscopes one night with one zone, one with two zones, and one of repose. This arrangement, which is the most convenient, was interrupted in April, by the reduction of our force to two assistants. The weather, which had for four or five months been indescribably bad—so much so indeed, that from the solstice to the March equinox there had been but five nights free from clouds—had just become settled and favourable; during an entire month there were but two cloudy nights, and until the middle of May we enjoyed magnificent opportunities for observing. During all this time Messrs. Thome and Bachmann continued the work without interruption or diminution, so that we observed one-half as many stars during this period of six weeks as during the whole six months preceding." Up to the date of Dr. Gould's report, August 1873, 379 zones had been observed, containing about 50,000 stars, of which less than one-fifth are duplicates. Dr. Gould adopted two degrees as the width of the zones as far as 46° south declination, gradually increasing the width southward until the last belt comprised the five degrees from 75° to 80° south declination. Near η *Argûs* and the constellation *Crux* it became necessary to subdivide the zones, owing to the richness of the galactic region in that neighbourhood. In this manner it is expected that more than 500 zones between the parallels of 23° and 80° south declination, containing about 65,000 stars, will be examined, which will require the greater part of another year to accomplish satisfactorily. Dr. Gould remarks that the labour of observation being so great, no time has been available for the arrangement or reduction of the observations, and that in fact the great majority of the records of the observations have not yet been read off from the sheets of the chronograph.

Photographs of Southern Star-clusters.

A series of southern star-clusters have been successfully photographed at the Argentine National Observatory, under the

direction of Dr. Gould. Although the circumstances under which the photographs were taken were very unfavourable, owing to the fracture of the flint-lens during the voyage into nearly two equal parts, yet some of the clusters are fairly represented in the photographs, but none of the stars give a trail, which prevents the zero of position being determined with the sharpness desirable. The method so successfully employed by Mr. Rutherfurd was adopted, the photographs being taken by Dr. Sellack, as follows: "After an exposure of the photographic plate for some minutes, the telescope, which was driven by a good clock-work, was moved to another position by the slow motion, and a second exposure was made; then the telescope was stopped and an exposure made, so that the image of the central star, while moving out of the field, leaves a trail. The images of the stars being double, the recognition and discrimination from specks produced by impurities of the plate is very much facilitated; the trail serves to fix a direction for the angles of position on the plate."

Of the photographs already taken the groups near ρ *Argûs* and Lacaille 4,375 are composed of about sixty stars; those of four other clusters, t^2 *Carinæ*, Lacaille 3,134, 4,145, and 7,478, contain about forty stars each. Altogether twenty-seven clusters have been photographed, including a total of about 800 stars. Since these cluster-pictures have been taken, Dr. Gould has, on his own responsibility, ordered a new object-glass, which has been delivered at Cordoba in perfect order. The Argentine Government having authorised the purchase of the lens for the Observatory, Dr. Sellack has been appointed a professor in the scientific faculty now organising in the University, with instructions to continue the photographic work for the present. Dr. Gould expects that he shall soon be able to report favourably of the results obtained.

New Double Stars.

Mr. Burnham, of Chicago, has contributed to the *Monthly Notices* three catalogues of hitherto unknown double stars which have been discovered by him, or which he has not been able to find in the different catalogues and publications relating to the subject. His first communication contains eighty-one new double stars, the second twenty-five, and the third seventy-six. In the course of last summer Mr. Burnham has been examining β *Delphini* with his 6-inch Alvan Clark refractor. This object is already known as a widely-separated double star, but he has now discovered that the principal component is itself double, too close, however, to be visibly separated with an instrument of 6 inches aperture, though viewed with a power of 410. Its duplicity appears to be certain, the secondary being about two magnitudes the smaller. The observed distance is only about 0″·5, and the position-angle is about 355°. Mr. Burnham is inclined to think

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that the distance is slightly under-rated, perhaps to the extent of one or two tenths of a second. He also observes that "it is a pretty difficult object for a 6-inch refractor; although within the last three months (August 1873) I have discovered a good many new double stars much more difficult, and one this same evening, when the components were at least 0″·2 closer." When *β Delphini* is again in a favourable position for observation it is to be hoped that observers possessing telescopes with larger aperture will carefully examine it, and report the result of their observations to the Society.

An examination of Mr. Burnham's catalogues shows that they contain several very interesting pairs of double stars which had escaped the notice of the two Herschels, the Struves, Dawes, and other double-star observers. Some of our large equatoreals might also be well employed in examining a selected few of these close doubles, so that their duplicity may be satisfactorily established beyond question, by a series of measures, when possible, of their angular distances and positions. In Mr. Burnham's catalogues these data are all obtained apparently from estimations only, probably owing to the difficulty of measuring such close objects; the estimations are therefore always liable to some degree of doubt, however careful the observer may be in recording his observations.

Variation of Position of Solar Spots.

In the *Proceedings of the Royal Society*, Messrs. De La Rue, Stewart, and Lœwy have given the results of an examination of the excess of spotted areas contained in the northern solar hemisphere over that of the southern, as observed on each day. In the course of the examination it soon became evident that during periods of considerable solar activity there is a tendency for the spots to change alternately from one hemisphere to the other. The period of this change is found to be about twenty-five days, while at times of little solar disturbance very little oscillation in the position of the spots is noticed. The three periods discussed extended from the beginning of August to the end of December 1859, from the end of June to the beginning of November 1860, and from the beginning of May to the end of August 1862. The respective values of a period of oscillation, by taking the mean of the positive and negative extremes, are 25·5 days, 23·3 days, and 27·2 days, giving for the whole three periods a probable mean value of 25·3 days. The authors do not profess to have discovered the cause of these oscillations, but they would, nevertheless, suggest that the observational facts here brought to light may perhaps be connected with two other observational facts, one of which was first noticed by Mr. Carrington, and the other by themselves.

"The first of these is the fact that, generally speaking, spots in the north hemisphere have much about the same latitude as those occurring at the same, or nearly the same, period in the

south, both sets widening or contracting together. It may, perhaps, therefore, be supposed, by applying this law, that the latitude of the spots which cause the positive extremes in the above series is not greatly different from that of those which cause the corresponding negative extremes.

"The second observational law is that which tells us that spots about the same period have a tendency to attain their maximum at, or near, the same ecliptical longitude. Now, if we suppose that in the foregoing three series the greatest positive extremes were caused by the positive spots attaining their greatest size, and the greatest negative extremes by the negative spots attaining their greatest size, it would follow that the two sets, positive and negative, must have taken their rise at places on the Sun's surface 180° of longitude different from each other, inasmuch as the one set, about twelve or thirteen days before or after, passed the same ecliptical longitude as the other.

"But if the positive set have the same latitude as the negative, and if the one is 180° of solar longitude different from the other, it would mean that *the two outbreaks are at opposite ends of the same solar diameter*. This conclusion is an interesting one, but of course it requires to be verified by further observation before it be finally received."

Radiation of Heat from the Moon.

The Earl of Rosse has presented a most important communication to the Royal Society on the Radiation of Heat from the Moon, the law of its absorption by our atmosphere, and its variation in amount with the lunar phases. It formed the subject of the Bakerian Lecture, read on March 27, 1873. Lord Rosse's early experiments have already been referred to in former Annual Reports, and astronomers generally will be glad to have an opportunity of possessing an abstract forwarded by him of his more complete series of observations, which form so valuable a contribution to lunar physics. In 1868 Lord Rosse first prepared an arrangement by which the heat in the image of the Moon could be measured, as formed by the mirror of the 3-foot reflector at Parsonstown, the details of which are given in the *Proceedings of the Royal Society* for 1869: "For the purpose of concentrating the heat of this image of 2·9 inches mean diameter on the face of a thermopile ⅓rd of an inch diameter, a concave mirror of 3½ inches diameter and 3 inches focal length was employed; a rock-salt lens being objectionable from its condensing moisture on its surface, and moreover, being hardly procurable of sufficient size. To secure greater steadiness of the needle, the two piles of four pairs each, which, having been made at *different* times, by Messrs. Elliott, were not of equal power, were replaced by two more equal thermopairs, constructed on the spot, fully described in the *Proceedings of the Royal Society* for 1870. The apparatus was enclosed on all sides, except on that towards the mirror of

the telescope, with a box of tin and glass, and the lattice-tube was covered with a cloth to keep draughts of air from the piles. Two covered wires led from the thermopiles to the galvanometer in the Observatory, and the heating effect was determined by directing the telescope so that the Moon's image fell alternately, for the space of one minute, on each of the two small concave mirrors.

"The observations made during the seasons 1868-69 and 1869-70 were found to follow pretty well Lambert's law for the variation of light with phase. It was found also that a piece of glass which transmitted 80 per cent. of the Sun's rays suffered only about 10 per cent. of the Moon's rays to pass through; thus a large amount of absorption before radiation from the Moon's surface was shown to take place.

"In the earlier experiments no attention had been paid to the correction to be applied for absorption of heat by the Earth's atmosphere; but, as the apparatus was gradually improved, it became indispensable to determine the amount of this correction before attempting to approach more nearly to the law of variation of the Moon's heat with her phases than had been done in the earlier investigation.

"By taking long series of readings for lunar heat through the greatest ranges of zenith distance available, a table expressing the law for decrease of heat with increase of zenith distance, closely following that deduced by Seidel for the corresponding decrease of the *light* of the stars, was obtained. By the employment of this table the determinations of the Moon's heat at various moments of the lunation were rendered comparable and available for laying down a more accurate "phase-curve" than had been previously obtained. This curve was found to agree more nearly with Professor Zöllner's law for the Moon's light, on the assumption that her surface acts as if it was *grooved* meridionally, the sides of the grooves being inclined at the uniform angle of 52° to the surface, than with Lambert's law for a perfectly *smooth* spherical surface.

"To exhibit the laws of absorption in the atmosphere, and of variation of heat and light, with the phases, the following abbreviated tables have been prepared:—

Zenith Distance	Light of Stars transmitted by Atmosphere	Moon's Heat transmitted by Atmosphere
0°	1·000	1·000
30	0·984	0·988
40	0·962	0·958
50	0·902	0·907
60	0·800	0·836
70	0·642	0·698
80	0·407	0·465
85	0·208	—

N.B.—Before entering the atmosphere the Moon's heat = 1·262, so that at the zenith fully $\frac{1}{5}$th is absorbed before it reaches the Earth's surface.

Distance from Full Moon	Lambert's Formula	Phase-curve for Heat (Observed)	Phase-curve for Heat transmitted by Glass	Curve representing Zöllner's Photometric Observations	Zöllner's Formula for Moon's light
100°	96	44	—	—	—
90	128	62	—	—	—
80	165	89	—	—	—
70	205	117	11·4	88	—
60	246	149	16·7	109	—
50	286	186	22·0	132	154
40	324	228	27·3	166	212
30	355	276	33·5	212	278
20	381	335	46·3	271	346
10	398	394	64·3	342	417
0	404	403	69·5	390	488
10	398	367	56·7	327	417
20	381	323	44·5	269	346
30	355	278	33·5	218	277
40	324	234	24·4	167	213
50	286	191	18·1	122	157
60	246	155	14·5	84	109
70	205	127	11·8	58	71
80	165	103	9·2	49	—
90	128	78	6·5	—	—
100	96	54	3·8	—	—
I.	II.	III.	IV.	V.	VI.

N.B.—To compare the heat transmitted by glass with Zöllner's photometric observations (column V.) the quantities in column IV. must be multiplied by 5·792.

"The distribution of light on two white globes, constructed in accordance with Lambert's and Zöllner's hypotheses, on which a beam from a strong light was thrown, may be shown to be very different in the two cases; the brightest spot on the former being at the centre, and on the latter at 52° on each side from the centre at the time of Full Moon, and at other times on the former at the bright limb, from which it gradually decreases towards the terminator; while on the latter there is a rapid decrease from the bright limb to a minimum about half-way to the terminator, after which it increases again, and then fades away on approaching the terminator.

"On examining the phase-curve which had been obtained, a certain want of symmetry on the two sides of Full Moon was perceived, which was ascribed to the unequal distribution of mountain and plain on the lunar surface. It had also been found that the percentage of the Moon's heat transmitted by a

sheet of glass diminished from 17·3 per cent. at Full Moon to about 13·3 per cent. at $22\frac{1}{2}°$, 11 per cent. at 45°, and 10 per cent. at $67\frac{1}{2}°$ distance from Full Moon; a circumstance which might have been accounted for by supposing that there is a constant amount of radiant heat coming from the Moon in addition to that which, like the light, varies with the phase, had it not been found that as the Moon approached tolerably near the Sun, as for instance, on March 27, 1871, when her distance from full was 138°, no perceptible amount of heat radiated from her surface.

"The less rapid decrease of the Moon's heat than of her light on going farther from Full Moon, and the increase of percentage of heat transmitted by glass towards the time of Full Moon, may probably be explained on the assumption that when the Sun's heat and light strike the Moon's surface the whole of the former and only a certain proportion of the latter, depending on the intrinsic reflecting power or 'Albedo' of the surface, leave it again; and consequently the shaded portions, which are inclined more towards the position of the Earth at Quadrature than at Full Moon, reflect a larger amount of heat as compared with the light at the former than at the latter time, and a greater flatness of the heat- than of the light- phase-curve is the result.

"With the view of obtaining a decisive result on the question, whether or not the Moon's surface requires an appreciable time to acquire the temperatures due to the various amounts of radiant heat falling on it at different moments, simultaneous determinations of the amount of the Moon's heat and of her light were made, whenever the state of the sky allowed of it, during the Eclipse of November 14, 1872. The Eclipse was a very partial one, only about $\frac{1}{40}$th of the Moon's diameter being in shadow; but although this circumstance, coupled with the uncertain state of the sky, rendered the observation far less satisfactory than it would otherwise have been, yet it was sufficient to show that the decline of light and heat as the penumbra came over the lunar surface and their increase after the middle of the Eclipse were sensibly proportional.

"Rather with the view of finding the value of the galvanometer readings in terms of the radiation from a surface of known temperature, and capable of being reproduced by anyone at any future time, than in the expectation of getting more than the roughest approximation to the temperature of the lunar surface, readings of the galvanometer were taken when the faces of the piles were alternately exposed for periods of one minute to the radiation from two blackened tin vessels filled with water of different temperatures. It was thus found that were the atmosphere removed, the surfaces of New and Full Moon might be respectively replaced by blackened tin vessels of equal apparent area to that of the Moon, and at temperatures of 50° and 247° Fahrenheit."

Motion of the Stars and Nebulæ in the Line of Sight.

Mr. Huggins has made some attempts to observe the motions of a few of the nebulæ in a similar manner to that so successfully adopted by him in his spectroscopic observations of the motions of stars in the line of sight, reference to which has been made in former Annual Reports. Although the results are negative, and thus show no decided proper motion of the nebulæ, still the experiment is not without considerable interest, for it sufficiently proves that the nebulæ examined cannot be moving in space in a direction towards the Earth with a velocity greater than thirty miles per second. Mr. Huggins has pointed out that there are three kinds of motion which may be supposed to exist in nebulæ, and which might possibly be detected by the aid of a spectroscope: a motion of rotation; another of translation in the visual direction of some portions of the nebulous matter contained within each nebula; and the third that which he had undertaken to search for, namely, a motion of translation in space of the whole nebula in the line of sight. The nebulæ examined by Mr. Huggins were selected from Sir John Herschel's catalogue. It will be highly important to continue this delicate inquiry by an examination of different classes of nebulæ, and it is therefore hoped that Mr. Huggins will further devote his attention to this very interesting and valuable application of spectrum analysis, which has already given him undoubted positive results of the amount of approach and recess of stars in the line of sight, results which, when combined with the ordinary observed proper motions of the stars, will give to us information of the true constitution of the heavens.

Some recent observations made by Dr. Vogel, who has followed the method adopted by Mr. Huggins, have strikingly confirmed the general results of stellar motion previously detected. He has experimented upon a few of the large stars and also on the nebula in *Orion*. He finds that *α Lyræ* has a motion towards the Sun amounting to 52 English miles per second, and *α Aquilæ* 48 miles in the same direction. The observed motion of the nebula of *Orion*, is, however, not so certain, but an apparent motion of about 17 miles per second receding from the Sun is indicated. Dr. Vogel had previously observed a motion of *Sirius* and *Procyon* receding from the Sun, amounting to 43 and 60 miles respectively.

Observation of Jupiter's Satellites.

The Council are glad to find that the observation of the phenomena of *Jupiter's* satellites has not been neglected. Since the Astronomer Royal's suggestion of the advisability of devoting an observatory for the special observation of these phenomena a

large number have been observed. At the Royal Observatory, so far as the more important meridional and altazimuth work will permit, these phenomena are always attended to, and each volume of the *Greenwich Observations* contains a long list of phemena of *Jupiter's* satellites observed during the year. The *Monthly Notices* for May and June 1873 also contain numerous observations made at Stonyhurst and the Radcliffe Observatory. Those at Stonyhurst were undertaken in answer to the suggestion of the Astronomer Royal, and it is expected that the observations will be continued there when practicable. As it is also the intention of Lord Lindsay to include the phenomena of *Jupiter's* satellites among the subjects for observation at his new Observatory at Dun Echt, we may confidently anticipate that, in a few years, a good accumulation of observations will be recorded. A great scarcity still exists of observations of the eclipses, transits, and occultations of the third and fourth satellites, and particular attention should therefore be given to these phenomena, as from their less frequent occurrence comparatively few have been hitherto observed; for it must be borne in mind that the tabular movements of the satellites cannot be corrected one by one, and that in any future revision of the theory of the motions of the satellites, the necessary relation among the motions of all four compels us to treat all together, in the manner pointed out by the Astronomer Royal in his paper on the subject.

Pendulum Operations at Kew in 1873.

During the latter half of the year Captain Heaviside, R.E., Deputy-Superintendent, Great Trigonometrical Survey of India, has been engaged in completing the observations for the formation at Kew of a base station for the pendulum operations which have been carried on in India since 1865. For this purpose the two invariable pendulums which were used in India have been swung at Kew, in order to determine whether any alteration has taken place in them since they were swung there by Mr. Loewy in 1865.

Observations are also being made with two convertible pendulums which have been used in Russia, and which have been lent by the Russian Government; these pendulums will give a value of the length of the simple seconds pendulum. Captain Heaviside also intends to swing Kater's original convertible pendulum at Kew, and a re-measurement of this pendulum will be undertaken at Southampton by Lieut.-Colonel Clarke.

Occultation of Regulus by the Planet Venus, A.D. 885.

The very interesting note by Mr. Hind on a reported occultation of *Regulus* by the planet *Venus*, A.D. 885, recorded in the manuscript

of the Arabian astronomer Ibn-Jounis, as described by Delambre in his *Astronomie du Moyen Age*, may be referred to as giving an example of the accuracy of the computed places from Le Verrier's tables of *Venus* at a distant epoch. Mr. Hind's calculations prove that the tables of *Venus* sufficiently represent the place of that planet so far back as one thousand years, as the time at which the observation taken from the old chronicle appears to have been made corresponds with that found from the tables for nearest approach. The record of a conjunction of *Mars* and *Venus* on February 13, 864, by the same astronomer, has been also examined by Mr. Hind, with a similar result.

Diameter of Venus.

A careful series of measures of *Venus* has been made by Mr. Plummer, at the Durham Observatory, using the double-image micrometer. Observations were made on twenty-six days near the inferior conjunction of the planet in 1873, and the resulting value of the diameter at mean distance is $17''\cdot321$, with a probable error of $\pm 0''\cdot046$, the augmentation of the diameter from the effects of irradiation being $-0''\cdot546$. It follows, therefore, that each contact is affected by one-half this quantity. Mr. Plummer has substituted these values in the equations, and on the whole the agreement is generally very good, and it is believed that in the cases where the difference is sensibly above the average that it arises from a variation in the amount of irradiation, depending upon the transparency of the atmosphere. The observations were all made in full daylight, when the planet was not far from the meridian.

Thomson's Manuscript Table of Logarithms.

In the course of the year a large MS. Table of Logarithms, constructed by the late Mr. John Thomson, of Greenock, has been presented to the Society, by his sister, Miss Catherine Thomson. The table, which is the result of an independent calculation, gives the logarithms to 12 places of all numbers from unity to 120,000, and therefore exceeds any printed table of logarithms of numbers in existence, and is only inferior to the *Tables du Cadastre*, which contain the logarithms of numbers to 200,000 to 14 or 15 places. The original 10-figure table from unity to 100,000 published by Vlacq in 1628 has never been extended (in print), except by the publication in 1801 of 11-figure logarithms of numbers from 100,000 to 102,000 by Borda and Delambre, though it has been reprinted, with a good many errors corrected, by Vega, in his *Thesaurus Completus* of 1794. The table calculated by Mr. Thomson gives, therefore, 18,000 additional logarithms, which have never been published, and also two more

figures (though the accuracy of the second must be questionable) of the logarithms in Vlacq and Vega. The MS. bears evidence of the care bestowed on its calculation, and it is apparently quite free from the errors in Vlacq and Vega; in fact, Mr. Thomson appears to have made no use at all of these works. The principle of the method employed by him seems to have been to calculate from certain fundamental logarithms, the logarithms of all the composite numbers up to the limit of the table, and then to interpolate for the higher primes, the whole being verified by second differences—a very good method when everything has to be performed by one person working only in his leisure-time.

It is probable that it will not be found desirable to publish so large a table as that calculated by Mr. Thomson, at all events for a long time, but its value is very great, as besides giving the logarithms of numbers beyond 102,000, it will be found very useful for the verification of Borda and Delambre's logarithms, and for testing any suspected error in Vlacq. But its chief utility will be experienced when a new reprint of Vlacq is undertaken, as it will then not only afford an important verification, but may perhaps also lead to the detection of a few errors that may have escaped M. Lefort in his comparison with the *Tables du Cadastre*.

Mr. Thomson was an accountant at Greenock, who died more than twenty years ago; and being in an infirm state of health for some years before his death, he left no directions with regard to the disposal of his tables, which have remained in the possession of his sister till their presentation by her to the Society. The MS. represents an enormous amount of painstaking labour, and it appears to be exceedingly accurate.

Mass of Jupiter.

Dr. H. C. Vogel has published a number of observations, made at Leipzig, of the distances of the third and fourth satellites of *Jupiter* from the planet, when near the times of their greatest elongations. They were made chiefly in the autumn and winter of 1868-9, but partly also in the winter of 1869-70, with the view of obtaining another determination of the mass of *Jupiter*, according to the method employed with so much success by Bessel and the Astronomer Royal. Herr Vogel has, however, not had time hitherto to make the requisite calculations for the determination of the mass, and contents himself for the present with giving the observations themselves, reduced so as to be able to test their accuracy. The third satellite was observed on sixteen evenings, and the fourth on eighteen evenings, both in the two positions of the instrument. The method of observation consisted in obtaining differences of right ascension between the satellite observed and both limbs of the planet by

means of a chronographic register. In the case of the third satellite the difference adopted was the mean deduced from twenty comparisons; in that of the fourth, from twenty-four comparisons on each evening of observation. The values of the diameter of the planet which incidentally result agree very satisfactorily with those calculated from the data of the *Berliner Jahrbuch*, viz., semi-major axis of *Jupiter* at the mean distance of the Sun from the Earth, 99″·8, and 0·928 for the proportion of the axes of *Jupiter's* ellipsoid. We may confidently look forward to a good determination of the mass of *Jupiter* from these observations, and are glad to see the method, which has been defined as "the readiest of all means to ascertain the correct value," again employed to determine the value of this important element in the solar system. Meanwhile reference may be made to Krüger's improved value of the mass obtained by him from the observations of the minor planet *Themis*, a notice of which is inserted in Vol. XXXIII. of the *Monthly Notices*, containing a comparison of Krüger's new mass of *Jupiter* with other well-determined values.

Struve's Companion of Procyon.

Induced by Otto Struve's remarkable observations of a small companion-star to *Procyon*, an abstract of which is given by Mr. Lynn in the last volume (XXXIII.) of the *Monthly Notices*, p. 430, Dr. Auwers has revised his interesting calculations of the motion of that star, availing himself of more recent observations than those employed in his former paper on the subject. He cannot, however, come to any decisive conclusion as to whether the star observed was really the cause which disturbs the motion of *Procyon*, but considers that the point may be settled with certainty should the small companion in question be seen again this year. If it turn out really to be so, some very interesting results will follow; for the star, being at a considerable distance from the centre around which *Procyon* is seen to move, and therefore from the common centre of gravity of the two, must be strictly a satellite of *Procyon*, and of much smaller mass; in fact, Dr. Auwers finds that the mass of *Procyon* would be about eighty times that of our Sun, while that of the companion would contain only about seven Sun-masses.

Brünnow's Researches on the Parallax of the Stars.

The second part of the Dunsink Observations contains further researches by Dr. Brünnow on the parallax of the stars, and is a valuable contribution to stellar astronomy. Dr. Brünnow deals with the parallax of five stars, *Groombridge* 1830, σ *Draconis*, 85 *Pegasi*, *Bradley* 3077, and α *Lyræ*. He considers that the

value of the parallax of the first-mentioned of these stars is still somewhat uncertain, although the results obtained by C. A. F. Peters, Wichmann, and O. Struve have established beyond doubt that it is considerably smaller than what might have been expected from its large proper motion, 7″·05 of a great circle. This uncertainty in the value of the parallax of *Groombridge* 1830 caused Dr. Brünnow to undertake a new determination, adopting as the comparison stars those named 2 and 6 in M. Otto Struve's paper, "Détermination de la Parallaxe de l'Etoile *Groombridge* 1830." The observations were commenced in January 1870, and were continued to January 1871, thus embracing an entire period of the parallax. Dr. Brünnow appears to be well satisfied with the final result obtained for this remarkable star, and he observes that "the value obtained for π from all the different combinations is so nearly the same, that I cannot consider the close agreement as merely accidental, but am inclined to accept it as a proof that the observations very decidedly exhibit such periodical deviations as a parallax of the magnitude of 0″·09 would produce." The resulting parallax for the remaining stars, as found from Dr. Brünnow's observations, is for σ *Draconis*, + 0″·25, which he considers to be a well-established value; for 85 *Pegasi*, + 0″·05; for *Bradley* 3077, + 0″·07, and for α *Lyræ*, + 0″·13. The star used for comparison with σ *Draconis* was a small preceding star of the 9·5 magnitude; 85 *Pegasi* was compared with a small companion star of the ninth magnitude, which does not participate in its proper motion; and *Bradley* 3077 and α *Lyræ* with neighbouring stars of the 9·5 magnitude.

The Constant of Nutation.

Dr. Magnus Nyrén, of the Pulkowa Observatory, has made use of a number of excellent observations made with the prime-vertical instrument, to determine anew the value of the Constant of Nutation. These observations were originally commenced by W. Struve, with that object in view, in the year 1840. They were, however, not carried on by him beyond 1855. O. Struve continued them in 1858 and 1859, and the remaining part of the series was observed in 1862 by Herr Oom, who now holds the office of Astronomer at Lisbon. The stars employed in this investigation were v *Ursæ Majoris*, ι *Draconis*, and o^2 *Draconis*, their proper motions being carefully determined by a long series of observations, commencing with Bradley's, at Greenwich, for the epoch 1755. The value of the nutation obtained by Dr. Nyrén is 9″·2437, with the probable error $\pm$ 0″·0118, for the epoch 1850. The mean of the values calculated by C. A. F. Peters and Lundahl, reduced to the same epoch, amounts to 9″·2207, with a probable error of $\pm$ 0″·0177.

Determination of the Solar Parallax from Observations of the Minor Planets.

Dr. Galle has proposed a method for determining the solar parallax by means of observations of the minor planets which approach nearest to the Earth. His suggestion was first made in a paper inserted in the *Astronomische Nachrichten*, Vol. LXXX., p. 1, with special reference to the opposition of *Phocea* in 1872, the method of observation to consist in measuring, with a wire-micrometer, equatoreal differences of declination between northern and southern stars, situated near the planet, simultaneously at observatories in the Northern and Southern hemispheres. With regard to *Phocea*, several series of observations were obtained in the Northern hemisphere near the time of opposition, using as comparison stars those selected for the purpose by Dr. Galle; but as no special intimation had been given to the Directors of our Southern observatories, no corresponding observations were made; hence much valuable labour was lost. During the past year, Dr. Galle being still convinced of the importance of a trial of this method, and noticing that the planet *Flora* would, in the autumn of 1873, be in a very favourable position for the determination of the solar parallax—the time of opposition coinciding exactly with that of perihelion passage, and the planet at the same time approaching the Earth within the distance of 0·87—prepared a list of comparison stars near *Flora*, the places being taken from the Bonn *Durchmusterung*, which has been published both in the *Monthly Notices* and in the *Astronomische Nachrichten*. Before this list, however, appeared, Dr. Galle had forwarded copies to Mr. Stone, Mr. Ellery, and Dr. Gould, at the same time drawing their attention to the plan of observation proposed by him. A tolerably fair series of observations have been made at the Cape, which will be available for comparison with the Northern observations, but the weather in Europe during the autumn has not been favourable for the accurate measurement of such minute objects. It is expected, however, that enough materials will be furnished to give a result sufficiently exact as a test of the accuracy of the method employed.

M. Le Verrier's Planetary Researches, and New Tables of Jupiter.

Another instalment of M. Le Verrier's great work on the theories of the major planets was communicated in July last to the Académie des Sciences, forming Section XXI. of his *Recherches Astronomiques*. It consists of an investigation of the theory of the planet *Saturn*, including the secular variations of the elements of its orbit; the smaller perturbations produced by *Venus* and the Earth; the periodical perturbations of the first and second order due to the action of *Jupiter*; the inequalities produced by

Uranus; the inequalities of the second order, which depend upon twice the mean motion of *Jupiter*, *plus* three times the mean motion of *Uranus*, *minus* six times the mean motion of *Saturn*; and, finally, the terms due to the action of *Neptune*. The complete theory of *Jupiter* had been previously communicated to the Academy in March 1873. In recording in the Annual Report the gradual progress of the herculean task which our valued Associate has imposed upon himself, the Council cannot avoid expressing their high appreciation of the importance of the researches which are now occupying his attention. It is to be hoped that at no distant date the results of his later planetary researches will be embodied in the practical form of tables of the motions of all the major planets, so that, by the continuous comparison of his theories with observation, a visible proof of the value of M. Le Verrier's labours may be exhibited, as has already been done with the planets from *Mercury* to *Mars*.

At a recent meeting of the Académie des Sciences, M. Le Verrier announced that he had completed his new tables of *Jupiter*, forming his fifth series of Planetary Tables. One more instalment is thus given towards the completion of the scheme proposed by him. The theory which has served as the basis of the tables of *Jupiter* has been developed in his four Memoirs presented to the Academy on May 20, August 26, November 11, 1872, and March 17, 1873. The theory has been compared with the observations of *Jupiter*, made at Greenwich, from 1750 to 1830, and from 1836 to 1869, and at Paris from 1837 to 1867. M. Le Verrier remarks that the theory and the observations are in complete accordance, and that there now remains no unknown body which appears to affect sensibly the motion of the planet in its orbit. The influence of the combined group of minor planets is perfectly insensible on *Jupiter*.

It is expected that these tables will soon be in the hands of astronomers. M. Le Verrier has informed Mr. Hind that they are not only completed, but that the printing has commenced, and is likely to be finished by the end of May. The tables are not in the ordinary form, but the alterations are such as will be duly appreciated by those engaged in their practical application, while presenting facilities for future reference to the theory. As was the case with the tables of the inferior planets, M. Le Verrier has promised Mr. Hind every assistance towards the immediate introduction of these important tables into the calculations of the *Nautical Almanac*.

New Tables of Uranus.

Professor Newcomb's important work on the orbit of *Uranus*, with general tables of the motion of that planet, was published in the autumn of last year; and although the President will explain

fully the general contents of the work, and the methods of treatment of the subject, the Council consider that a record of Professor Newcomb's investigation, with its accompanying tables, so much needed, should appear also in this section of the Report. The idea of investigating the theory of the motion of *Uranus* seems to have occupied the attention of Professor Newcomb so far back as 1859, his object at first being little more than tentative efforts to obtain numerical data of sufficient accuracy for his purpose, and to decide upon a satisfactory method of computing the general perturbations of the planet. He found that the elements of *Neptune* employed in the earlier computations were too inaccurate to be used in the calculations of the perturbations of *Uranus*, even to the first order of accuracy, and it was necessary, therefore, before entering into any detailed investigation, to correct them. This was done in 1864 and 1865, and the investigation on the orbit of *Neptune* was printed by the Smithsonian Institution in the latter year. Considering all this work as preliminary, the investigation of the orbit of *Uranus* was begun in earnest in 1868, and has occupied Professor Newcomb during the greater part of the succeeding five years.

The tabular places of *Uranus* in the American *Nautical Almanac* for 1876 have been contributed in manuscript to that work. Through the kindness, also, of Professor Newcomb, the places of *Uranus* in the British *Nautical Almanac* for 1877 are also derived from the new tables.

Secular Variations of the Elements of the Planetary Orbits.

A very valuable contribution to mathematical astronomy is contained in the last published volume of the *Smithsonian Contributions to Knowledge*. It consists of an elaborate memoir by Mr. J. N. Stockwell on the secular variations of the elements of the eight principal planets, with tables of the elements, together with the obliquity of the ecliptic, and the precession of the equinoxes in both longitude and right ascension. This investigation was commenced about twelve years ago, and it has occupied the attention of Mr. Stockwell at intervals during the succeeding ten years. The memoir is divided into five sections: 1. On the secular variations of the excentricities and perihelia. 2. On the secular variations of the nodes and inclinations of the orbits. 3. On the position and secular variations of the orbits when referred to the invariable plane of the planetary system. 4. On the precession of the equinox, and the obliquity of the ecliptic. 5. Tabular values of the elements of the planetary orbits.

On examining the works of mathematicians who had previously investigated this problem, Mr. Stockwell considered "that the methods of reducing to numbers those analytical integrals which determine the secular variations of the ele-

ments were far from possessing that elegance and symmetry of form which usually characterise the formulæ of astronomy. The first step, therefore, was to devise a system of algebraic equations, by means of which we should be enabled to obtain the values of the unknown quantities with the smallest amount of labour. It was soon found to be impracticable to deduce algebraic formulæ for the constants, by the elimination of eight unknown quantities from as many linear symmetrical equations, of sufficient simplicity to be used in the deduction of exact results. It therefore became necessary to abandon the idea of a direct solution of the equations, and to seek for the best approximate method of obtaining rigorous values of the unknown quantities. This has been accomplished as completely as could be desired, and by means of the formulæ which have been obtained it is now possible to determine the secular variations of the planetary elements with less labour, perhaps, than would be necessary for the accurate determination of a comet's orbit." The numerical coefficients of the differential equations having been afterwards computed, they have been substituted in these equations, and, by means of successive approximations, the rigorous values of the constants corresponding to the assumed masses and elements have been deduced. The details of the computation, as well as a tabulation of the various planetary elements of sufficient extent to supply the needs of the astronomer, will be found in the memoir.

New Method of Treating the Lunar Theory.

In a paper printed in the *Monthly Notices*, Vol. XXXIV., No. 1, Sir G. B. Airy has shown that the result of rejecting (on the authority of M. Delaunay and Professor Newcomb) the term in the Lunar Theory, arising from the indirect action of *Venus* on the Moon, is a discordance between theory and observation during the last 120 years, which is utterly inadmissible, and from which he is led to the conclusion that there is still some serious defect in the Lunar Theory. With a view of removing this, he has, in a very important paper, read at the January meeting, pointed out a method by which the approximate solution of the equations of motion obtained by M. Delaunay may be verified, and corrections applied to it, if necessary, so that any required degree of precision may be attained, and this without requiring that incessant personal attention to the laborious details of the computation which has hitherto been necessary. This grand work has already been commenced, and much progress made in it, with the aid of a computer who is intrusted with the arithmetical computations, under the personal direction of the Astronomer Royal.

Annales de l'Observatoire de Paris.

Volume X. of this important work has just been issued, under the direction of M. Le Verrier. The preceding volume appeared in 1868, but owing to the change in the management of the Paris Observatory the series was suspended. On February 13, 1873, the new Council of the Observatory, at its first meeting, resolved that the publication of the *Annales* should be resumed. This volume contains the memoir presented to the Académie des Sciences on August 26, 1872, relating to the mutual actions of *Jupiter* and *Saturn*, and a memoir by Messrs. Wolf and André, relating to the black-drop and other singular phenomena which are generally observed at internal contacts of *Mercury* and *Venus* during transits of these planets over the Sun's disk. Volume XI. of the *Annales* is in the press, and a new volume of the series containing the *Observations* is also in the press, together with a continuation of the ecliptic charts.

Progress of Meteoric Astronomy during the Year 1873.

The progress of observations and discussions has not been so marked during the past year by any signal occurrence, like that in 1872, of an important meteoric shower, nor by any remarkable occurrences of aërolites and fireballs which would ordinarily excite attention; yet the additions made to this department of astronomy have continued to progress, and to contribute towards a more perfect knowledge of meteoric systems. Observations of the annually recurring meteoric showers have been repeated, at the Royal Observatory, Greenwich, at the Radcliffe Observatory, Oxford, at Stonyhurst College, and at some private stations, from whence information of their appearance has been communicated to the Luminous Meteor Committee of the British Association; while the same activity in recording the appearances of shooting-stars and meteorites in foreign countries continues, as before, to contribute most highly to advance this modern branch of physical astronomy.

The two November star-showers of 1872, and the bright shower of August meteors in 1871, were thus observed in Italy with excellent results. The shower of *Leonids* was seen at Matera on the morning of the 15th of November, 1872, returning with very distinct intensity; and the *Perseids*, in 1871, were, like the *Andromedes* of the following year, observed at most of the Italian stations to exceed considerably the ordinary scale of their annual frequency and brightness. The shower was almost equally conspicuous on the nights of the 10th and 11th of August in both of the years 1871 and 1872; and its unusual

brightness in 1871, as compared with a marked maximum of the shower in 1863, agrees in indicating an eight-year period in the fluctuations of this shower, which has already been noticeable in the dates of its minimum returns from the time of its first discovery in the year 1836, until its last inconsiderable appearance in 1870. The radiant-point of the shower in 1871 was pretty accurately ascertained to be at about R.A. 35°, N. decl. 59°, and the time of maximum in 1871 and 1872, between the 10th and 11th of August, differed, as well as the radiant point, materially from the time and direction of the Earth's passage through a stream of particles following exactly the orbit of Tuttle's Comet, which were announced by Mr. Hind, a few days before the latter shower, to be, for the year 1872, at about 11h. P.M. on the 9th of August, and at a point in the constellation *Perseus*, occupying a position in R.A. 51°, N. Decl. 52°, about nine degrees from the observed direction of the radiant-point. Separate ramifications of the shower may perhaps account for considerable variations, both in the time and of the direction of divergence of the August shower on successive occasions of its annual appearance. It was thus also recently suggested as a possible explanation of the double shower of the *Andromedes* observed on the 24th and 27th of November, 1872, that the former outlying stream may have been the attendant meteor-current of the telescopic Comet discovered by Pogson, and generally regarded as a visible companion of the missing comets of Biela in that year, while the later and principal shower was, without doubt, similarly connected with the two missing nuclei of the comet. The brightness of the principal nuclei Schiaparelli supposes may be so variable (as has sometimes been less distinctly the case with other comets), that their present disappearance may be only temporary, and that future observations may succeed in rediscovering their existence. Not the smallest semblance of a return of either of the two Biela star-showers of 1872 is now known to have been visible in 1873, wherever darkness and clear nights favoured the expectations of observers who watched diligently for their reappearance at the latter end of November and the beginning of December last; and the vast tract of its orbit, which the meteor-cloud of 1872, like the Comet, describes from perihelion in a single year, readily accounts for their total absence, where the meteors have not yet formed a closed ring about the cometary path.

The appearances of the *Leonids* have also very sensibly decreased in recent years. Since their last widely-observed return in 1869, the shower has only been visible on exceptionally favourable opportunities at a few points of observation. It occupied the two mornings of the 14th and 15th of November 1870, with a slight and nearly equal intensity on both those dates; and it presented distinct but by no means conspicuous maxima of frequency on the mornings of the 15th and 14th of November respectively, in the years 1871 and 1872. On the

last annual date of its expected return in 1873 it escaped observation, as far as accounts have yet appeared, entirely; and the main body of this shower appears, like that of Biela's Comet, to have withdrawn itself completely from further observations.

Since the first determinations of their radiant-point in the years 1862 and 1863, and another striking appearance of the meteors of December 12 in the year 1866, the brightness of this shower has undergone very slight variations, and some good observations of the *Geminids* were obtained on the 12th of December last, during a somewhat unusual frequency of their appearance in 1873. A former pretty abundant appearance of the meteors of the 2nd of January in 1863 and 1864, when their radiant-point was first observed, having failed to be visible again for several years, it was yet re-observed with some brightness on the night and morning respectively of the 2nd of January in 1872 and 1873; and, partly on account of cloudy weather, intended observations of its return on the 2nd of January in the present year afforded no indications of a recurrence of this shower. The annual shower-meteors of October 18–21, and of April 19–21, have recently been visible at intervals at the Radcliffe Observatory, at Oxford, and at a few other English stations, without presenting any great abundance of these meteors sufficient to distinguish their display as unusual, since a somewhat conspicuous occurrence of these showers respectively in the years 1863 and 1864. The principal dates of their observation were on the nights of October 21st, 1871, April 20th, 1872, and October 20th, 1873. The annual appearances of these showers, although variable, appears seldom to be entirely interrupted, and a special interest will now attach to their continued observation, from the results of recent investigations of their earlier occurrences which have lately been pointed out by Professor Kirkwood.

With the exception of the January star-shower, all the above annual meteor-systems are found to be so exactly connected in date with certain ancient and recent great star-showers as to afford decided indications of a nearly uniform periodic cycle in their returns, which, in the case of the October, April, and December star-showers, varies from $27\frac{1}{2}$ to $29\frac{1}{8}$ years, associating them very nearly, in their greatest elongation from the Sun, with the orbit of the planet *Uranus*. A similar cycle of thirteen years is found to connect together some former and recent appearances of the January star-shower, which in this case indicates a connection of that meteor-system with the less distant orbit of the planet *Saturn* as the probable source of its introduction into the Solar system. Of two comets (1792 II. and 1860 IV.) shown by Dr. Weiss to have approached the Earth's orbit very nearly at the point which it passes on the 4th of January, neither present elements which can be identified with those of the meteoric-stream, the radiant-point of the star-shower being at R.A. 234°, N. Decl. 51°, and that of the Comet 1792 II., which approaches it most nearly, being at R.A. 216°, N. Decl. 14°. But should the periodi-

cal return of the January shower be found to correspond in future with the most recent exhibitions of this shower in January 1835–40, and 1862–65, it is probable that during the years 1875–79 considerable recurrences of these meteors on the 2nd of January will be observed.

Another meteor-system apparently often presenting itself in former years is found by Professor Kirkwood to correspond in present date with meteor-showers of the last two or three days of April or the 1st of May. Six ancient star-showers recorded in M. Quetelet's Catalogue between A.D. 401 and A.D. 1009, corresponding with this date, afford indications of a short cycle of not quite seven years in their occasional returns. Among recent observations of meteor-showers, the extensive list presented by Captain Tupman in the *Monthly Notices* of this Society for March last, contains some notes of a fine star-shower observed on each of the nights of the 30th of April and 2nd of May 1870, of which a satisfactory position of the radiant-point was also obtained on the 29th of April 1869. The observed positions of the radiant-point scarcely differ more than two or three degrees from each other and from the average place of the three observations at R.A. 326°, S. Decl. 2½°, a point only 20° from the apex of the Earth's way; and the hourly frequency of the shower (reduced to the zenith) for one observer was 15 or 20 meteors on the different nights. In Pingré's Catalogue of cometary orbits the elements of an ancient comet are given which was visible for about one month in Europe, in April 1006, the path of which, at the descending node, passes at a short distance (about $\frac{1}{12}$th of the Earth's mean distance from the Sun) outside of the Earth's orbit. The radiant-point corresponding to the parabolic track of this comet, supposing the Earth to encounter any of its particles at their descending node, is in R.A. 334°, S. Decl. 4°, about 8° from the place observed in the star-showers of April and May 1869 and 1870; and the point of the Earth's passage through the node corresponds to the date when these meteor-showers were seen. The elements of the meteor-shower of April 30th, 1870, and of the Comet, may be thus compared:

Comet of A.D. 1006.		Meteor-shower of April 30, 1870.		
Perihelion Passage, March	12	Perihelion Passage, April	1	
Longitude of Perihelion	304°	Longitude of Perihelion	280°	
Longitude of Ascending Node	38°	Longitude of Ascending Node	38°	Parabolic Paths.
Inclination	17½°	Inclination	22°	
Perihelion Distance	0·583	Perihelion Distance	0·737	
Motion Retrograde		Motion Retrograde		

If the meteor-shower of April 16, 1009, was connected with the Comet of 1006 and followed in its path, its perihelion passage would have happened on the 7th of March, but five days earlier

in date than the perihelion passage of the Comet, which must therefore have passed through its node only five days after the Earth had passed through the nearest point of its orbit, about eight millions of miles from the Comet at that point. Thus the nearest approach of the Comet to the Earth scarcely exceeded twelve millions of miles; and if a small comet, its visibility for a short time at that return may be explained by its close approach to the Earth, which may not have occurred again, should its orbit really be elliptical, and its later returns have been occasionally repeated. But the position of this Comet's perihelion, nearly midway between its ascending and descending nodes, does not favour the supposition that it belongs to the class of periodical comets whose aphelia are near the orbits of either of the major planets. On the other hand, the small obliquity and retrograde motion of the orbit considered in connection with the remarkable apparent fixity of the node since the first recorded appearance of the April and May star-shower on the 9th of April, A.D. 401 (corresponding to the 29th of April, 1870), appears to indicate that if the comet of 1006, and the shooting-stars observed in 1869 and 1870 are identical with it, the orbit of this meteor-stream can scarcely be one of short period, and it may not impossibly be a portion of a long parabolic stream of successive meteor-clouds and comets drawn from some distant region of space, and extended into a continuous meteor-current by the Sun's attraction. Fresh observations of shooting-stars at the end of April and on the first days of May will, however, be of important value to ascertain if the star-shower recorded in Captain Tupman's observations presents a seven years' cycle of periodicity in its returns, like that which is assigned to the shower-meteors of this date by Professor Kirkwood.

Under the superintendence of the Luminous Meteor Committee, the British Association have sanctioned the separate publication of a complete Catalogue of Captain Tupman's observations, and the useful materials which they will afford to observers of shooting-stars will now be placed at their disposal by the liberality with which the British Association continue to regard the pursuit and advancement of this branch of astronomy, and which they have been pleased to extend to this important contribution to meteoric science, as a groundwork for further exact investigations of the principal characters and positions of meteoric systems.

Papers Read before the Society from February 1873, *to February* 1874.

1873.

Mar. 14. Meteoric Shower, November 27, 1872. Mr. Graham.

Ditto Ditto Capt. Chimmo.

New Use of the Altazimuth Diagram. Rev. A. Freeman.

Markings on *Venus*. Mr. Wilson.

On an Instance of Abnormal Refraction. Rev. J. Slatter.

On the Eclipses mentioned in the Saxon Chronicle. Rev. S. J. Johnson.

Phenomenon observed at Sea. Captain Knevitt.

On Meridian Marks for Transit Instruments. Mr. Crossley.

On the Barometric Error of Clocks. Mr. Webster.

On Mr. Denison's Compensation for ditto. Rev. Dr. Robinson.

On the want of Observations of Eclipses of *Jupiter's* first Satellite from 1868 to 1872. Sir G. B. Airy, K.C.B.

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Note on an Astrolabe. Mr. Lecky.

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Observations of Faye's Comet at Marseilles. M. Stéphan.

Note on the Position of the Small Stars near ε *Lyræ*. Mr. Wilson.

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ADDRESS

Delivered by the President, Professor Cayley, on presenting the Gold Medal of the Society to Professor Simon Newcomb.

The Council have awarded the medal to Professor Simon Newcomb for his Researches on the Orbits of *Neptune* and *Uranus*, and for his other contributions to mathematical astronomy. And upon me, as President, the duty has devolved of explaining to you the grounds of their decision.

I think it right to remark, that it appears to me that in the award of their highest honour the Council of a Society are not bound to institute a comparison between heterogeneous branches of a science, or classes of research—to weigh, for instance, mathematical against observational astronomy, or astronomical physics; or, in the several branches respectively, the happy idea which originates a theory against the patience and skilled labour which developes and carries it out; and still less to decide between the merits of different workers in the science. It is enough that the different branches of a science coming before them in different years, the medal should in every case be bestowed as a recognition of high merit in some important branch of the science.

Before speaking of the Tables, I will notice some of Professor Newcomb's other works.

Memoir "On the secular Variations and mutual Relations of the Orbits of the Asteroids," *Mem. American Academy*, Vol. V. pp. 124–152 (1860). The object is to examine those circumstances of the forms, positions, variations, and general relations of the asteroid orbits which may serve as a test, complete or imperfect, of any hypothesis respecting the cause from which they originated, or the reason why they are in a group by themselves. Every *à posteriori* test is founded on the supposition, that the hypothesis necessarily or probably implies that certain conditions must be satisfied by the asteroids or their orbits, viz., in the one case the conditions are those which follow necessarily and immediately from the hypothesis itself, in the other case those which are deducible from it by the principle of random distribution. The two principal hypotheses are that of Olbers, where the asteroids are supposed to be the fragments of a shattered single planet, and the hypothesis that they were formed by the breaking up of a ring of nebulous matter. On the first hypothesis the orbits of all the asteroids once intersected in a common point; the second affords no conclusion equally susceptible of an *à posteriori* test.

But for a rigorous or probable test of either hypothesis, what is needed is rigorous expressions in terms of the time

for the excentricity, inclination, and longitudes of perihelion and node of each of the asteroids considered, or, what is the same thing, the computation of the secular variations of the quantities h,l,p,q, which replace these elements. The investigation is applied to those asteroids the elements of which were determined with sufficient accuracy, and the excentricities and inclinations of which were sufficiently small (limit taken is 11°). And the backbone of the memoir is the investigation of the h,l,p,q, for twenty-five asteroids included between the numbers (1) and (40). In this calculation, as was clearly necessary, the action of the asteroids on the larger planets and on each other was neglected; the expressions for the h,l,p,q, of the larger planets are regarded as given—they are, in fact, taken from Le Verrier (as calculated by him before the discovery of *Neptune*, but afterwards partially extended to that planet). The effect is that the differential coefficients $\frac{dh}{dt}$, &c. are given each of them as a sum of sines or cosines of arguments varying with the time; and thus, although the calculation is sufficiently laborious, the process is not one of the extreme labour and difficulty which it is in the case of the larger planets. The resulting table of the h,l,p,q, of the twenty-five asteroids has, of course, a value quite independent of the theoretical part of the memoir. Of this it is sufficient to say here that the conclusion is on the whole against Olbers's hypothesis. The subject is resumed, and more fully examined in a paper in the *Astronomische Nachrichten*, t. 58.

"Investigation of the Distance of the Sun and of the Elements which depend upon it, from the Observations of *Mars* made during the Opposition of 1862, and from other Sources," *Washington Observations for* 1865, Appendix II., pp. 1-29. The chief part of this valuable Memoir is occupied with a determination of the solar parallax by the discussion of the observations of *Mars* made in 1862 on the plan of Winnecke: three partial discussions had previously appeared, but these having been by comparisons of pairs of observations, one in each hemisphere, many observations in one hemisphere were lost by want of a corresponding observation in the other hemisphere; and out of a total of nearly 300 observations, only 125 were utilised. The idea is, the perturbations of the Earth and *Mars* being perfectly known for the period under consideration, every observation of the planet would lead rigorously to an equation of condition between its parallax, the six elements of its orbit, and the six elements of the Earth's orbit—thus 13 or more observations, when compared with any theory, should suffice to correct the errors of that theory. But the observations extending only over a short interval, say one month, the coefficients would be so minute as to give no trustworthy value of the corrections; the equations only suffice to determine a few functions of the elements, which being determined, the equations will be satisfied by widely differing

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values of the elements, if only these values are such as to give to the functions their right values. And by fixing *à priori* on the entire number of functions in question, and using them in place of the elements of the Earth and *Mars*, the equations will be practically as rigorous as if all the 13 unknown quantities had been introduced. By such considerations as these, each observation is made to give a relation between only 3 unknown quantities, the correction of the Sun's parallax being one of them.

The principle appears to be one of extended application, in regard to the proper mode of dealing with the constantly recurring problem of the determination of a set of corrections from a large number of linear equations; and it is used by the author in regard to the equations which present themselves in his theories of *Neptune* and *Uranus*.

Returning to the *Mars* observations, these were made at 6 Northern and 3 Southern Observatories, the total number being 154 Northern, and 143 Southern, together 297 observations. There was the difficulty of reducing to a concordant system the observations at the different Observatories, since (the whole number of comparison stars not being observed on each night) the adopted mean position of each of them was not unimportant. But this being carefully discussed and allowed for, the observations, extending from August 21 to November 3, 1862, are divided into five groups, and from these is deduced a correction to the provisional value $8''\cdot9$ of the parallax. The author then reproduces or discusses other determinations, from micrometic observations of *Mars*, the parallactic inequality of the Moon, the lunar equation of the Earth, the transit of 1769, and Foucault's experiment on Light—the last result, as not a strictly astronomical one, and with no means of assigning its probable error, is left out of consideration—and the combination of the remaining ones gives the author's concluded value of the parallax; from which other astronomical constants are deduced.

"On the Right Ascensions of the Equatoreal Fundamental Stars and the Corrections necessary to reduce the Right Ascensions of different Catalogues to a mean homogeneous System." *Washington Observations for* 1870, Appendix III., pp. 1–73.

This important Memoir is referred to in the Council Report for 1873. The object is to do for the right ascensions of the equatoreal and zodiacal Stars what had been done by Auwers for the declinations, namely, to furnish the data necessary to reduce the principal original catalogues of stars to a homogeneous system by freeing them of their systematic differences. The results are contained in two tables of corrections (as depending on the R.A. and N.P.D. respectively) to the several catalogues; and in a table of concluded mean right ascensions for the beginning of each fifth Besselian year, 1750 to 1900, of 32 fundamental Stars, and of periodic terms in the right ascensions of *Sirius* and *Procyon*.

The evil of systematic differences between the observations of different Observatories of course presents itself in every case

where such observations have to be combined: for instance, in the just-mentioned determination of the solar parallax by the observations of *Mars*; and in the making of a set of planetary tables: and all that tends to remove or diminish it is most important to the progress of Astronomy. I cannot help thinking that there should be some confederation of Observatories, or Central calculating Board, for publishing the lunar and planetary observations, &c., reduced to a concordant system. It seems hard upon the maker of a set of planetary tables that he should not at least have, ready to hand for comparison with his theory, a single and entire series of the observations of the planet.

"Théorie des Perturbations de la Lune, qui sont dues à l'action des Planètes." *Liouv.* t. xvi (1871) pp. 1-45. This is a very important theoretical Memoir on the disturbed motion of three bodies: a problem which, so far as I am aware, has not hitherto been considered at all. I have elsewhere remarked that the so-called "Problem of Three Bodies," as usually treated is not really this problem at all, but a different and more simple one—that of disturbed elliptic motion. Thus, in the planetary theory, each planet is considered as moving in an ellipse, and as disturbed by the action of forces represented by means of a disturbing function peculiar to the planet in question. An approach is made to the problem of three bodies when, as in memoirs by Hamilton and Jacobi, the (say) two planets are replaced by two fictitious bodies, and instead of a disturbing function peculiar to each planet, the motion of the system is made to depend on a single disturbing function. And there are memoirs by Jacobi, Bertrand, and Bour, which do relate to the proper problem of three bodies, viz. to their undisturbed motion. But in the present Memoir, Professor Newcomb starts from this problem as if it were actually solved, viz. he takes the coordinates of the three bodies (Sun, Earth, and Moon) as given in terms of the time and of 18 constants of integration.* And then considering the system as acted upon by the attraction of a planet, represented by means of a disturbing function, he applies to the system of the three bodies the method of the variation of the elements. The six elements which determine the motion of the centre of gravity of the system are left out of consideration; there remain to be considered 12 elements only; six of these are $\epsilon_0, \pi_0, \theta_0, \epsilon'_0, \pi'_0, \theta'_0$ (initial mean longitudes and longitudes of pericentre and node): but the other six k_ϵ, k_π, &c., are functions the invention of which is a leading step in the theory, and it is in fact by means of them that the

* Of course the expressions actually used must be approximations: the centre of gravity of the Earth and Moon is regarded as moving round the Sun in an ellipse affected by a secular motion of perihelion (ultimately neglected); and the coordinates of the Moon in regard to the Earth are considered to be given by Delaunay's Lunar Theory. The centre of gravity of the whole system (in the undisturbed motion) moves uniformly in a right line, viz., the coordinates are $a+a't$, $b+b't$, $c+c't$; and we have thus the whole number $6+6+6, =18$, of arbitrary constants.

investigation is brought to a successful conclusion: the expressions of the last-mentioned six functions can, it is stated, be formed with facility by means of the developments (obtainable from the lunar theory) of the rectangular coordinates x, y, z, as periodic functions of the time. With these twelve elements, the expressions for the variations assume the canonical form

$$\frac{dk_\epsilon}{dt} = \frac{dR}{d\epsilon_0}, \ \frac{d\epsilon_0}{dt} = -\frac{dR}{dk_\epsilon}, \ \&c.$$

The concluding part of the Memoir contains approximate calculations which seems to show that the whole process is a very practicable one: but the author remarks that it is only doing justice to Delaunay to say that, starting from his (Delaunay's) final differential equations, and regarding the planet as adding new terms to the disturbing function, there would be obtained equations of the same degree of rigour as those of his own Memoir.

Everything in the Lunar Theory is laborious, and it is impossible to form an opinion as to the comparative facility of methods; but irrespectively of the possible applications of the method, the Memoir is, from the boldness of the conception and beauty of the results, a very remarkable one, and constitutes an important addition to Theoretical Dynamics.*

I come now to the planets *Neptune* and *Uranus*: it is well known how, historically, the two are connected. The increasing and systematic inaccuracies of Bouvard's Tables of *Uranus* were found to be such as could be accounted for by the existence of an exterior disturbing planet; and it was thus that the planet *Neptune* was discovered by Adams and Le Verrier before it was seen in the telescope, in September 1846. It was afterwards ascertained that the planet had been seen twice by Lalande, in May 1795. The theory of *Neptune* was investigated by Peirce and Walker: viz., Walker, by means of the observations of 1795, and those of 1846-47, and using Peirce's formulæ for the perturbations produced by *Jupiter*, *Saturn*, and *Uranus*, determined successively two sets of elliptic elements of the planet. The values first obtained showed that it was necessary to revise the perturbation-theory, which Peirce accordingly did, and with the new perturbations and revised normal places, the second set of elements (*Walker's Elliptic Elements II.*) was computed. With these elements and perturbations there was obtained for the planet from the time of its discovery a continuous ephemeris,

* Since the above was written, Professor Newcomb has communicated to me some very interesting details as to the extent to which he has carried his computations, and in particular he mentions that, considering the action of each planet from *Mercury* to *Saturn*, he has (in regard to the terms the coefficients of which might become large by integration) estimated the probable limiting value of more than fifty such terms of period from a few years to several thousands without finding any which could become sensible, except the term leading to Hansen's first inequality produced by *Venus*.

published in the *Smithsonian Contributions*, *Gould's Astronomical Journal*, and since 1852 in the *American Ephemeris* and the *Nautical Almanac*. The theory was next considered by Kowalski in a work published at Kasan in the year 1855. The long period inequalities are dealt with by him in a manner different from that adopted by Peirce, so that the two theories are not directly comparable, but Professor Newcomb, by a comparison of the ephemerides with observation, arrives at the conclusion that the theory of Kowalski (although derived from observations up to 1853, when the planet had moved through an arc of 16°) was on the whole no nearer the truth than that of Walker; he observed, however, that this failure is accounted for by an accidental mistake in the computation of the perturbations of the radius vector by *Jupiter*.

Professor Newcomb's theory of *Neptune* is published in the *Smithsonian Contributions* under the title "An Investigation of the Orbit of *Neptune*, with General Tables of its Motion," (accepted for publication, May 1865). The errors of the published ephemerides were increasing rapidly; in 1863 Walker's was in error by 33″, and Kowalski's by 22″; both might be in error by 5′ before the end of the century. The time was come when (the planet having moved through nearly 40°) the orbit could be determined with some degree of accuracy. The general objects of the work are stated to be:

(1) To determine the elements of the orbit of *Neptune* with as much exactness as a series of observations extending through an arc of 40° would admit of.

(2) To inquire whether the mass of *Uranus* can be concluded from the motion of *Neptune*.

(3) To inquire whether these motions indicate the action of an extra-Neptunian planet, or throw any light on the question of the existence of such planet.

(4) To construct general tables and formulæ, by which the theoretical place of *Neptune* may be found at any time, and more particularly between the years 1600 and 2000.

The formation of the tables of a planet may, I think, be considered as the culminating achievement of Astronomy: the need and possibility of the improvement and approximate perfection of the tables advance simultaneously with the progress of practical astronomy, and the accumulation of accurate observations; and the difficulty and labour increase with the degree of perfection aimed at. The leading steps of the process are in each case the same, and it is well known what these are; but it will be convenient to speak of them in order, with reference to the present tables: they are *first* to decide on the form of the formulæ, whether the perturbations shall be applied to the elements or the coordinates—or partly to the elements and partly to the coordinates; and as to other collateral matters. These are questions to be decided in each case, in part by reference to the numerical values (in particular the ratios and

approach to commensurability of the mean motions), in part by the degree of accuracy aimed at, or which is attainable—the tables may be intended to hold good for a few centuries, or for a much longer period. The general theory as regards these several forms ought, I think, to be developed to such an extent, that it should be possible to select, according to the circumstances, between two or three ready-made theories; and that the substitution therein of the adopted numerical values should be a mere mechanical operation; but in the planetary theory in its present state, this is very far from being the case, and there is always a large amount of delicate theoretical investigation to be gone through in the selection of the form and development of the algebraical formulæ which serve as the basis of the tables. In Prof. Newcomb's theory the perturbations are applied to the elements; in particular it was determined that the long inequality arising from the near approach of the mean motion of *Uranus* to twice that of *Neptune* (period about 4,300 years), should be developed as a perturbation, not of the coordinates, but of the elements. And it was best, (as for a theory designed to remain of the highest degree of exactness for only a few centuries) to take not the mean values of the elements, but their values at a particular epoch during the period for which the theory is intended to be used. The adopted provisional elements of *Neptune*, and the elements of the disturbing planets, are accordingly not mean values, but values affected by secular and long inequalities, representing the actual values at the present time. *Secondly*, the form being decided on and the formulæ obtained, the numerical values of the adopted provisional elements of the planet, and of the elements of the disturbing planets and their masses, have to be substituted, so as to obtain the actual formulæ serving for the calculation of a provisional ephemeris; and such ephemeris, first of heliocentric, and then of geocentric positions, has to be computed for the period over which the observations extend. *Thirdly*, the ephemeris, computed as above, has to be compared with the observed positions; viz. in the present case these are, Lalande's two observations of 1795, and the modern observations at the Observatories of Greenwich, Cambridge, Paris, Washington, Hamburg, and Albany, extending over different periods from 1846 to 1864: these are discussed in reference to their systematic differences, and they are then corrected accordingly, so as to reduce the several series of observations to a concordant system. In this way is formed a series of 71 observed longitudes and latitudes (1795, and 1846 to 1864); the comparison of these with the computed values shows the errors of the provisional ephemeris. *Fourthly*, the errors of the provisional elements have to be corrected by means of the last-mentioned series of errors: as regards the longitudes, the comparison gives a series of equations between $\delta\epsilon$, δn, δh, δk, and μ (correction to the assumed mass of *Uranus*). The discussion of the equations shows that no reliable value of μ can be obtained from them; it indeed appears that if *Uranus* had been unknown,

its existence could scarcely have been detected from all the observations hitherto made of *Neptune* (far less is there any indication to be as yet obtained as to the existence of a trans-Neptunian planet): hence, finally, μ is taken = 0, and the equations used for the determination of the remaining corrections. As regards the latitudes, the comparison gives a series of equations serving for the determination of the values of δp and δq. And applying the corrections to the provisional elements, the author obtains his concluded elements; viz. as already mentioned, these are the values, as affected by the long inequality, belonging to the epoch 1850. *Fifthly*, the tables are computed from the concluded elements, and the perturbations of the provisional theory.

After the elements of *Neptune* were ascertained, the question of its action on *Uranus* was considered by Peirce in a paper in the *Proc. American Acad.* Vol. I. pp. 334-337 (1848). This contains the results of a complete computation of the general perturbations of *Uranus* by *Neptune* in longitude and radius vector, but without any details of the investigation, or statement of the methods employed: it is accompanied by a comparison of the calculated and observed longitudes of *Uranus* (with three different masses of *Neptune*) for years at intervals from 1690 to 1845, and for one of these masses the residuals are so small that it appears that using these perturbations by *Neptune*, and Le Verrier's perturbations by *Jupiter* and *Saturn*, there existed a theory of *Uranus* from which quite accurate tables might have been constructed. But this was never done. The ephemeris of *Uranus* in the *American Ephemeris* was intended to be founded on the theory, but the proper definitive elements do not seem to have been adopted: and in the *Nautical Almanac* for the years up to 1876, Bouvard's Tables of *Uranus* were still employed; for the year 1877 the ephemeris is derived from heliocentric places communicated by Prof. Newcomb.

An extended investigation of the subject was made by Safford, but only a brief general description of his results is published, *Monthly Notices*, *R.A.S.*, vol. xxii. (1862.) The effect of *Neptune* was here computed by mechanical quadratures; and corrections were obtained for the mass of *Neptune* and elements of *Uranus*.

Professor Newcomb's Tables of *Uranus* have only recently appeared. They are published in the *Smithsonian Contributions* under the title "An Investigation of the Orbit of *Uranus*, with General Tables of its Motion," (accepted for publication February, 1873); forming a volume of about 300 pages. The work was undertaken as far back as 1859, but the labour devoted to it at first amounted to little more than tentative efforts to obtain numerical data of sufficient accuracy to serve as a basis of the theory, and to decide on a satisfactory way of computing the general perturbations. First, the elements of *Neptune* had to be corrected, and this led to the foregoing investigation of that planet: it then appeared that the received elements of *Uranus* also differed too

widely from the truth to serve as the basis of the work, and they were provisionally corrected by a series of heliocentric longitudes, derived from observations extending from 1781 to 1861. Finally, it was found that the adopted method of computing the perturbations, that of the "variation of the elements" was practically inapplicable to the computation of the more difficult terms, viz., those of the second order in regard to the disturbing force. While entertaining a high opinion of Hansen's method as at once general, practicable, and fully developed, the author conceived that it was on the whole preferable to express the perturbations directly in terms of the time, owing to the ease with which the results of different investigations could be compared, and corrections to the theory introduced; and under these circumstances he worked out the method described in the first chapter of his treatise, not closely examining how much it contained that was essentially new. With these improved elements and methods the work was recommenced in 1868; the investigation has occupied him during the subsequent five years: and, though aided by computers, every part of the work has been done under his immediate direction, and as nearly as possible in the same way as if he had done it himself: a result in some cases obtained only by an amount of labour approximating to that saved by the employment of the computer.

The leading steps of the investigation correspond to those for *Neptune*: there is, *first*, the theoretical investigation already referred to; *secondly*, the formation of the provisional theory with assumed elements; *thirdly*, the comparison with observation; and here the observations are the accidental ones previous to the discovery of *Uranus* as a planet by Herschel in 1781, and the subsequent systematic ones of twelve Observatories, extending over intervals during periods from 1781 to 1872; all which have to be freed from systematic differences, and reduced to a concordant system as before: the operation is facilitated by the existence, since 1830, of ephemerides computed from Bouvard's Tables serving as an intermediate term for the comparison of the observations with the provisional theory. *Fourthly*, the correction of the elements of the provisional theory, viz., the equations for the comparison of the longitudes give $\delta\epsilon$, δn, δh, δk, and a correction to the assumed mass of *Neptune*, which mass is thus brought out $=\frac{1}{19840}$. And the equations for the comparison of latitudes give δp, δq; there is thus obtained a corrected set of elements, (Newcomb's Elements IV.), being for the year 1850, the elements as affected with the long inequality; these are the elements upon which the Tables are founded. But it is theoretically interesting to have the absolute mean values of the elements, and the author accordingly obtains these (his Elements V.) together with the corrections corresponding to a varied mass of *Neptune*, (that is, the terms in μ corresponding to a mass $\frac{1+\mu}{19700}$); he remarks that, admitting the mass of *Neptune* to be uncertain by about one fiftieth of its value, the

mean longitude of the perihelion of *Uranus* is from this cause uncertain by more than two minutes, the mean longitude of the planet by nearly a minute, and the mean motion by nearly two seconds in a century. *Fifthly*, the formation of the tables, based on the Elements IV.; the tables calculated with these elements are intended to hold good for the period between the years 1000 and 2200; but by aid of the Elèments V. they may be made applicable for a more extended period.

In what precedes I have endeavoured to give you an account of Professor Newcomb's writings: they exhibit all of them a combination, on the one hand, of mathematical skill and power, and on the other hand of good hard work—devoted to the furtherance of Astronomical Science. The Memoir on the Lunar Theory contains the successful development of a highly original idea, and cannot but be regarded as a great step in advance in the method of the variation of the elements and in theoretical dynamics generally; the two sets of planetary tables are works of immense labour, embodying results only attainable by the exercise of such labour under the guidance of profound mathematical skill—and which are needs in the present state of Astronomy. I trust that imperfectly as my task is accomplished, I shall have satisfied you that we have done well in the award of our medal.

The President then, delivering the medal to the Foreign Secretary, addressed him in the following terms:

Mr. Huggins—I request that you will have the goodness to transmit to Professor Newcomb this medal, as an expression of the opinion of the Society of the excellence and importance of what he has accomplished; and to assure him at the same time of our best wishes for his health and happiness, and for the long and successful continuation of his career as a worker in our science.

The Meeting then proceeded to the election of the Officers and Council for the ensuing year, when the following Fellows were elected:—

President.

J. C. Adams, Esq., M.A., F.R.S., Lowndean Professor of Astronomy, Cambridge.

Vice-Presidents.

Sir G. B. Airy, K.C.B., LL.D., F.R.S., &c., Astronomer Royal.
Arthur Cayley, Esq., M.A., F.R.S., Sadlerian Professor of Geometry, Cambridge.
Rev. Robert Main, M.A., F.R.S., Radcliffe Observer.
Rev. Charles Pritchard, M.A., F.R.S., Savilian Professor of Astronomy, Oxford.

Treasurer.

Samuel Charles Whitbread, Esq., F.R.S.

Secretaries.

Edwin Dunkin, Esq.
A. Cowper Ranyard, Esq., M.A.

Foreign Secretary.

William Huggins, Esq., D.C.L., LL.D., F.R.S.

Council.

W. H. Mahony Christie, Esq., M.A.
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J. W. Lee Glaisher, Esq., B.A.
Captain J. Herschel, R.E., F.R.S.
T. A. Hirst, Esq., Ph.D., F.R.S.
George Knott, Esq.
William Lassell, Esq., F.R.S.
Lord Lindsay, M.P.
Captain William Noble.
Rev. S. J. Perry.
Captain G. L. Tupman, R.M.A.
J. Maurice Wilson, Esq., M.A.

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXXIV. *March* 13, 1874. No. 5.

PROFESSOR ADAMS, F.R.S., President, in the Chair.

George Russell Rogerson, Esq., Waterloo, near Liverpool, and
Richard Cross, Esq., Forest Hill,

were balloted for and duly elected Fellows of the Society.

The following Resolution was passed at the last meeting of the Council:—

Resolved:—That all books belonging to the Society, now in the hands of Fellows, be returned to the Assistant-Secretary at Somerset House, on or before the 1st of May next, after which date, and until further notice, no book can be taken out of the Library without the express permission of the Library Committee.

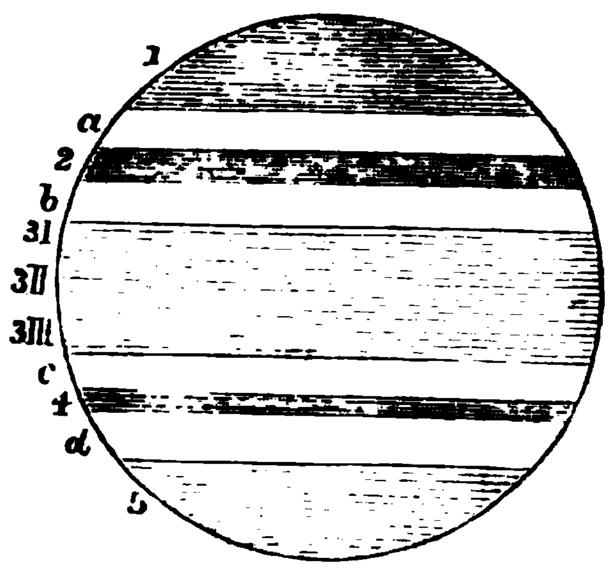
Notes to accompany Chromolithographs from drawings of the planet Jupiter, made with the Six-foot Reflector at Parsonstown, in the years 1872 and 1873.

(*Communicated by the Earl of Rosse, F.R.S.*)

Much attention having been given of late to the appearance of the belts and other markings on *Jupiter*, more especially in regard to their colour, and the employment of the pencil having been facilitated by the application of a clock-movement to our Six-foot Reflector, we were induced last season to try how far the more delicate markings and tints on the planet's surface, which might

be beyond the reach of smaller instruments could be brought out by a larger aperture. The disadvantage which the limited range* of that instrument in right ascension entailed in the delineation and continuous study of the physical features of a rapidly revolving planet, together with the circumstance that more advantage is derived from increase of aperture in the examination of *fainter* objects, had been the cause of most, possibly too large a proportion, of our attention having been directed from the first to the Nebulæ. In the short space of time available, it was found impossible to do more than take hasty sketches at the telescope accompanied by notes of the colours and general appearance of the belts. From these sketches water-colour drawings, of which the accompanying lithographs are faithful copies, were made, generally on the following morning. To render each drawing as independent as possible of any preconceived bias, the colours were invariably mixed afresh, and in no case was a drawing compared with any of those already made until it was completely finished. The drawings have been executed by Dr. Copeland, usually observing alone. The whole of the notes are given as they were written down at the telescope. Although many of these may perhaps appear to be unnecessary, as only expressing in words what is given in the drawings, a careful comparison will show that there are many points of divergence between drawing and description, so that the one may be regarded as to some extent supplementing the other. There are also many nuances of colour and peculiarities of form represented so faintly that they might readily be overlooked or considered as accidental unless special attention was drawn to them. From the fact already mentioned, that each sketch was coloured without any reference to the others, it is scarcely to be wondered at that colours which are named alike in the notes, show widely different tints in the drawings.

To make the notes intelligible without the necessity of inserting letters or other marks in the drawings, the following nomenclature was adopted. The dark zones are marked 1, 2, 3, 4 and 5, as shown in the figure, and the bright ones *a*, *b*, *c* and *d*. In the case of the equatoreal zone, 3, the Roman numerals I, II and III, are appended to express subdivisions; in all cases the smaller numbers and earlier letters refer to the southern or upper part of the disk.



Unfortunately the drawings have not been arranged, as we had intended, in regular order of the rotation of *Jupiter*, owing to our having been misled by an error in

* At most, 40 minutes for an object near the Equator.

Sir John Herschel's *Outlines* (Art. 512, p. 340, 8th edition), where the period of $9^h\ 55^m\ 50^s$ mean time is marked sidereal time.

The error was only detected, after the lithographs had been struck off, on reading Dr. Schmidt's article in No. 1973 of the *Astronomische Nachrichten* and then refering to No. 1541 of that journal. The correct values of L, the Jovicentric longitude of the true centre of the planet's disk, are given below. After the correction the drawings gain much in interest from the possibility of tracing a brick-red marking through many revolutions. The zero of longitude or first meridian of *Jupiter* is that which *Jupiter* would have presented to the Earth at noon on December 31, 1872, had his geocentric longitude been the same as at the moment of the opposition of 1873. The time at which the light left *Jupiter* is also given for each observation.

Beer and Mädler's period of rotation, $9^h\ 55^m\ 26^s{\cdot}5324$ mean time*, has been used in calculating the values of L.

Arrangement of Sketches in order of the Rotation of Jupiter.

No.	Date (Greenwich mean time).			h	m	L. °	Time when light left *Jupiter*. h	m
9	1873	Apr.	1,	9	49	3·0	9	10·0
14	„	Apr.	11,	8	58	38·2	8	18·3
10	„	Mar.	13,	10	59	63·4	10	21·9
6	„	Feb.	7,	13	34	72·5	12	57·6
5	„	Jan.	26,	14	27	95·4	13	50·0
17	„	Mar.	11,	11	13	130·3	10	35·7
16	„	Feb.	27,	12	5	153·7	11	28·8
12†	„	Feb.	10,	13	21	156·7	12	44·3
8	„	Jan.	24,	14	36	159·1	13	58·5
19	„	Feb.	20,	12	36	197·3	11	59·9
15	„	Feb.	3,	13	52	200·1	13	15·1
7	1872	Dec.	31,	16	15	201·4	15	36·1
11	1873	Jan.	17,	15	11	205·1	14	33·9
18	„	Feb.	8,	13	30	220·6	12	53·2
13	„	Jan.	22,	14	44	222·8	14	7·1
3	„	Apr.	10,	9	12	256·1	8	32·4
2	„	Mar.	7,	11	33	259·9	10	56·3
1	„	Feb.	6,	13	38	284·4	13	2·0
4	„	Mar.	22,	10	21	316·0	9	42·7

The ratio—

Polar semidiameter = Eq. semidiameter × 0·927—

was adopted from the *Nautical Almanac*, for the form of the

* See *Beiträge zur physischen Kenntniss der himmlischen Körper im Sonnensysteme*, by Wilhelm Beer and Dr. J. H. Mädler. Weimar 1841.

† No. 12, the lithograph is wrongly dated January 22.

disk; the equatoreal diameter of the drawings is 50 millimètres.

The magnifying power most used was 414; but in disturbed states of the atmosphere a lower power of 281 or even 120 often gave better definition, with these however only 4 ft. 8 in. and 2 ft. respectively of the large mirror came into use.* On rare occasions power 650 could be used with advantage.† As all markings on the surface of the planet when situated near the edge of the disk have always been noted by us as being excessively faint and dim, little attention was paid to any traces of phase, and no attempt has been made to delineate it.

Notes on the Colours and general Appearance of the Belts.

Opposition of 1872.

January 15, 1872, $12^h\ 55^m$ Greenwich mean time. 1 grey; a yellow patch at the northern end of the dark marking in 2; 3 yellow, there is also a narrow streak of yellow on the northern edge of 4; 5 bluish. The planet was at times wonderfully well defined with power 414.

February 7, 1872, $11^h\ 12^m \pm$ G.M.T. 1 of a very uniform grey tint; white patches on the margin of the yellow equatoreal belt; 4 of a purplish neutral tint. Sketch very hurried.

Opposition of 1873.

No. 7; December 31, 1872, $16^h\ 15^m$ G.M.T. $L = 201^\circ\!\cdot\!4$.

In 3 I and 3 III are dark clouds; there is a bright diagonal rift in 3 III, and a bright central area in 3 II. The tints noted are: 1 greyish; *a* white; 2 greyish; *b* very white; 3 yellowish red; 4 the darkest part of the disk, and lastly 5 of a pale ashy blue. The equatoreal belt is not nearly so red as it was in the spring of 1872. The central area of 3 II is about the brightest part of the disk. 5 is darker than 1. There is a streak of yellow along the northern edge of 4.

No. 11; January 17, 1873, $15^h\ 11^m$ G.M.T. $L = 205^\circ\!\cdot\!1$.

b is very white and breaks in upon the equatoreal belt near the following (right) side, forming a white gulf, following which is a yellowish brown patch; *c* is white. The equatoreal belt is not nearly so intensely coloured as it was last year, the centre being almost colourless—grey and white—but bounded north and south

* It is assumed that the aperture of the pupil was ¼ inch, probably too large an estimate in the case of so bright an object as *Jupiter*. On no occasion were diaphragms used to reduce the aperture of the telescope, nor indeed is any provision made for their application.

† Powers 650, 281, and 414 are achromatic positive eye-pieces; the two former by Mr. H. Grubb, the latter by the late Mr. Cooke; their performance is very good in all parts of the field.

by more highly coloured regions. Sat. I as seen to emerge from behind the planet was of a deep yellow colour (shown in the drawing) and much fainter than the planet. There is a marked projection in the middle of the south side of the equatoreal belt. Definition good for a few very short intervals, otherwise rather poor.

No. 13; January 22, 1873, $14^h\ 44^m$ G.M.T. L = 222°·8.

1 yellowish; a reddish or reddish-yellow patch on the following side of *b*; a number of large and small, very white spots in 3 II; the northern polar zone (5) of a very uniform slate-grey.

No. 8; January 24, 1873, $14^h\ 36^m$ G.M.T. L = 159°·1.

The sky very hazy, with definition in rare moments only. Nothing more than a rough and hurried sketch taken. The tints are, 1 yellowish; *a* white; *b* very white; *c* very white; 4 purplish and the darkest part of the disk, and lastly 5 of a bluish grey.

No. 5; January 26, 1873, $14^h\ 27^m$ G.M.T. L = 95°·4.

The large, black spot is the shadow of Satellite III. Definition mostly poor, but still the large oblique white patches on the equatoreal belt are occasionally well seen. The darkest patch is a little south of III Sh. Belt 2 fades away on the following side; zone 5 is as usual bluish grey.

No. 15; February 3, 1873, $13^h\ 52^m$ G.M.T. L = 200°·1.

The preceding and following parts of 3 I are reddish, the former inclining to brown and the latter to a very red copper colour: large white clouds in 3 II, small ones and a rather long white streak in 3 III; northern pole bluish grey. The northern edge of 3 is very ragged. Definition occasionally good, otherwise very poor; the long, narrow, grey cloud seen very well repeatedly, as also the bend and shading of belt 2, but the equatoreal spots are not so well defined as on other occasions. The Sat. III noted as being 1½ time the diameter of Sat. II.

No. 1; February 6, 1873, $13^h\ 38^m$ G.M.T. L = 284°·4.

1 yellowish with a whitish patch on the preceding side; *b* very white on the following, reddish yellow on the preceding side: *c* very white; 4 neutral tint—darkest part of the disk—bounded by a narrow, yellowish line on the north; 5 blue-grey. Powers 120, 281, and 414. The following features were noted as being remarkable: 1st, the *right-handed* inclination of the cloud crossing the Equator; 2nd, the immense reddish yellow area extending far to the south on the preceding side, and 3rd, the fact of belt 2 being, so to say, *forced up* into contact with 1. There is only the slightest separation (not shown in the lithograph) of the preceding part of the dark boundary of 1 from the rest, in the other

part 2 is completely connected with 1, which is decidedly yellow in contrast with the blue of 5. Belt 4 is much darker than usual, with the neutral tint of last year and the band of yellow on the north. The patches on 3 are of a pale brownish yellow.

No. 6; February 7, 1873, $13^h\ 34^m$ G.M.T. L = 72°·5.

Definition occasionally good; power 281. *b* very white; 3 yellowish with white patches; 5 greyish. The belt 2 is much darker than usual, being nearly the darkest part of the disk; its following end, which is bent towards the north and terminates before reaching the limb, is separated from the equatoreal belt by a very white interval that seems to be composed of rounded cloudy masses. The south edge of 3 is formed of two fine parallel lines of dark cloud, the other markings being very irregular with large 'port-holes.'

No. 18; February 8, 1873, $13^h\ 30^m$ G.M.T. L = 220°·6.

A reddish yellow patch on the following side of *b;* the dark spot in 3 II is of a full blue colour, while the white spots are intensely white and bright. The middle of *b* is composed of white clouds; *d* very white; *c* also white, but not quite so much so as *d;* 5 bluish grey. 3 III exhibits a long line of very small white clouds. The driving clock very troublesome (owing to recent alterations), many clouds are also passing over, but between them the definition is remarkably good, and all the markings drawn are well seen. There is no mistake about the blue colour of the spot in 3 II, as it was repeatedly examined in all parts of the field of view. When the clouds pass over the planet it is at once evident that the centre of the disk is much brighter than the margin.

No. 12; February 10,* 1873, $13^h\ 21^m$ G.M.T. L = 156°·7.

b very white; 3 I reddish brown, as also 3 III; large white patches on 3 II, the brightest of which, near the preceding side of the disk, is the brightest marking on the planet; 5 slate colour. Seen very well for a few seconds between clouds; the clouds never completely obscured the planet, so that as their increasing density obscured one part after another, the order of brilliancy of the various regions of the disk could be readily estimated. Tested in this way the north polar zone (5) would seem to be about the darkest part of the disk, for when the planet is nearly obscured that zone is much darker than any other region. The belt 4 disappears rather on account of its narrowness than its blackness, so that the depth of its shading cannot be compared with that of the broader belts by this method. The central part of the equatoreal belt is on the whole very bright, but the spot already mentioned, on the preceding

* The lithograph is wrongly dated January 22.

side, is considerably the brightest part, remaining visible as a bright oval disk when the rest of the planet is nearly obscured.

No. 19; February 20, 1873, 12^h 36^m G.M.T. $L = 197^\circ\cdot3$.

b is composed of white clouds; there are also a number of larger and smaller white clouds on 3 II and 3 III; 3 I is reddish, and *d* very white; while 5 is noted as dingy; 4 is as usual the darkest part of the disk. Almost all the red colour is now confined to the southern part of the equatoreal belt, the rest of which is much broken up; the dark marking well seen—not blue. A small dark object passed rapidly (almost instantaneously) across the planet from north—preceding to south—following (Query, a meteorite?).

No. 16; February 27, 1873, 12^h 5^m G.M.T. $L = 153^\circ\cdot7$.

The belt 2 is ragged on both edges, and has a number of very small white clouds on it; 3 I is reddish, with exceedingly small white clouds; 3 II and 3 III of a very faint yellow, with very white clouds, the black spot is the shadow of Sat. II; 4 is purplish, with a yellowish streak on the northern edge; 5 is bluish.

The dark lines on the equatoreal belt are fully darker than 4. The south polar zone, which is about half the width of the northern one, is at times suspected to be rough on the edge. Definition only very rarely good. Powers 281 and 414.

No. 2; March 7, 1873, 11^h 33^m G.M.T. $L = 259^\circ\cdot9$.

a and *b* are very white, the following part of *b* is reddish; 3 II and 3 III show white patches.

No. 17; March 11, 1873, 11^h 13^m G.M.T. $L = 130^\circ\cdot3$.

1 very faint; 2 is composed of very white and very irregular clouds; 3 I is reddish and 3 III yellowish; on the preceding side of 3 is a large bluish grey marking; 5 greyish blue; 4 the darkest part of the disk. The north polar zone was not seen bluish at first, but on closer examination it is certainly not yellow. Definition rarely good; all the markings, except the two dark streaks, are very faint.

No. 10; March 13, 1873, 10^h 59^m G.M.T. $L = 63^\circ\cdot4$.

1 is very faint; in 2 there are two patches darker than the rest; *b* and the patches in 3 are very white, the following patch in 3 II being the brightest; 3 I is yellowish; in 3 there are two bluish spots, of which the following one is the darker; 4 is again the darkest part of the disk, and of a neutral tint. Definition rather good.

No. 4; March 22, 1873, $10^h\ 21^m$ G.M.T. L = 316°·0.

a very white; 1 yellowish; there is a reddish spot on the preceding side of 2 and *b*; an excessively white patch in 3 II, also a dark blue spot in the same zone; 4 is as usual neutral tint; *d* very white, and 5 bluish. Definition pretty good, with powers 281 and 414.

No. 9; April 1, 1873, $9^h\ 49^m$ G.M.T. L = 3°·0.

Belt 2 is plainly interrupted; there is a reddish streak in 3 I, as also a dark streak certainly visible in *c*; belt 4 is very narrow.

No. 3; April 10, 1873, $9^h\ 12^m$ G.M.T. L = 256°·1.

b is very white, with a reddish patch on the following side; the patches in 3 I are very white; 4 is the darkest part of the disk—neutral tint; there is a blue patch in 3 II, and one of a darker blue tinge in 3 III; 5 is bluish grey. Definition very good, with powers 281 and 414. The inclined streak with its bend is very well shown. The red patch in *b* seems to break off abruptly on the following side. There is a narrow band of red on the northern edge of 3. The irregularities in 2 are very well seen; 4 is very narrow, and thought to be bordered with red. The inclined belt and the southern edge of 3 are composed of parallel dark streaks, as shown in a drawing previously made (compare No. 13, January 22).

No. 14; April 11, 1873, $8^h\ 58^m$ G.M.T. L = 38°·2.

2 of a purplish or dark neutral tint; *a*, *b*, 3 I, parts of 3 II and 3 III, as also *c* and *d*, are white; the dark cloud in 3 III is bluish, 4 is purplish, and 5 bluish grey. Definition at times almost perfect, with powers 281, 414, and 650.

Before attempting to deduce any results from the foregoing observations, it may be well to say a few words about two published sets of drawings of *Jupiter* made contemporaneously with our own. The first of these is contained in "Observations de *Jupiter* et de *Mars* faites à Louvain pendant l'opposition de ces planètes en 1873; par M. F. Terby, docteur en sciences.—Extrait des *Bulletins de l'Académie royale de Belgique*, 2me série, tome XXXVI., No. 11, 1873." There are 14 drawings of *Jupiter*, made with a telescope of 9 centimètres ($3\frac{1}{2}$ in.) aperture, and powers 120, 186, and 240. Assuming the dates given to be Louvain mean time, we have reduced them to Greenwich mean time, using $18^m·8$ for the difference of meridians, and have calculated the values of L to facilitate comparison.

M. Terby's No.	Greenwich M.T.	L.	Time when light left *Jupiter*.
	h m	°	h m
T 1	1873, Jan. 23, 10 6·2	205·4	9 29·1
„ 2	„ Feb. 28, 8 3·7	158·3	7 27·1
„ 3	„ Mar. 8, 6 23·7	223·4	5 46·7

M. Terby's No.	Greenwich M.T.					L.	Time when light left *Jupiter*.
				h	m	°	h m
T 4	1873,	Mar.	8,	8	3·7	283·9	7 26·7
„ 5	„	„	23,	6	21·2	321·8	5 43·1
„ 6	„	„	24,	8	6·2	175·9	7 28·0
„ 7	„	„	25,	6	26·2	266·0	5 47·9
„ 8	„	„	25,	7	58·7	322·0	7 20·4
„ 9	„	„	26,	7	48·7	106·6	7 10·3
„ 10	„	„	27,	7	56·2	261·8	7 17·7
„ 11	„	„	31,	7	38·7	133·7	6 59·9
„ 12	„	Apr.	1,	6	16·2	234·4	5 37·3

Nos. 13 and 14 we have omitted, as they only profess to represent the transit of the 4th Satellite.

The other set of drawings are those published by Mr. Knobel, in *Monthly Notices*, vol. xxxiii. p. 474; these are three in number, for which we find the following quantities:

Mr. Knobel's No.	Greenwich M.T.					L.	Time when light left *Jupiter*.
				h	m	°	h m
K 1	1873,	Mar.	26,	7	50	107·4	7 11·6
„ 2	„	Apr.	20,	7	30	259·7	6 48·9
„ 3	„	May.	11,	8	15	207·3	7 31·2

The drawings were made with a silver-on-glass reflector of 8½-in. aperture, and a power of 208.

Of all the features presented to our view by *Jupiter* during the opposition of 1873, probably the most remarkable is the great break in the southern side of the equatoreal belt in L=260°. It is shown in Nos. 2, 3, 7, 11, 13, 15, and 18, it occurs most unmistakeably in K 2 and 3, and is also indicated by the markings in T 3 and 4, and perhaps to some extent also in T 7, 10, and 12. On December 31, 1872 (No. 7), it would seem that the break had either not fully occurred, or that it was very imperfectly seen, being 35° following the centre of *Jupiter's* disk; on January 17, 1873, it was fairly seen. On January 22 and April 10 (Nos. 13 and 3), it was seen to great advantage under most favourable circumstances; on both these days the preceding part of the southern edge of the equatoreal belt consisted of two parallel lines of dark objects (clouds?) separated by a clearer interval, but with this remarkable difference, that whereas on the former occasion both lines were curved towards the Equator at their following ends, on the latter, only the southern one was so curved, thus causing it to coalesce with, and possibly to intersect the other. This union appears to have been very permanent, for it was well seen by Mr. Knobel on April 20 and May 11. Following and filling up this break is a brick-red area, that was seen most fully on February 6 (No. 1), the chromolithograph for which day is, however, too faintly tinted. The red region may extend

some 30° in longitude, reaching from L 250° to L 280°: its following end is just seen in sketch 4.

Since the break in the edge of the equatoreal belt is indicated by the following end of the long, curved marking in our earliest drawing, and is fully shown by the intersection of the dark belts in Mr. Knobel's latest drawing, and was depicted with more or less certainty on eight other occasions, there seemed to be a chance of determining the period of rotation from it with some considerable accuracy. Taking therefore the Parsonstown Sketches 2, 3, 7, 11, 13, 15 and 18, also K 2, K 3, and T 3; further, using the times when the light left *Jupiter* on each occasion (as a means of correcting for the varying distance of the planet), also allowing *in time* for the distance of the point selected for observation from the centre of the planet's apparent disk, and lastly making a still further small time-allowance for phase, the following summary was arrived at:—

	Corrected time when the point was at the true centre of the planet's disk. 1873. G.M.T. d	h	m	Rotations elapsed.
	0	16	34·7	0
	17	15	23·8	41
	22	15	5·0	53
	34	14	44·9	82
	39	14	23·0	94
	66	11	0·4	159
T 3	67	5	43·1	161
	100	8	45·8	241
K 2	110	7	25·6	265
K 3	131	8	41·7	316

Putting then t = the error, in minutes of time, of the first observation, and Δ R = error of Mädler's period, and combining the first observation with each of the others in succession, we have the following equations of condition:—

$$
\begin{aligned}
0 &= -\overset{\mathrm{m}}{4{\cdot}0} + t + 41\ \Delta R\\
0 &= +31{\cdot}9 + t + 53\ \Delta R\\
0 &= +23{\cdot}9 + t + 82\ \Delta R\\
0 &= +56{\cdot}7 + t + 94\ \Delta R\\
0 &= +30{\cdot}4 + t + 159\ \Delta R\\
0 &= -37{\cdot}8 + t + 161\ \Delta R\\
0 &= +29{\cdot}5 + t + 241\ \Delta R\\
0 &= +58{\cdot}7 + t + 265\ \Delta R\\
0 &= +7{\cdot}3 + t + 316\ \Delta R
\end{aligned}
$$

The complete solution of these equations gives $t = -18^{\mathrm{m}}{\cdot}44$

$+ 14^{m}\cdot15$; $\Delta R = -0^{m}\cdot02172 \pm 0^{m}\cdot07758$ and the probable error, r, of a single observation $= \pm 21^{m}\cdot65$. Substituting the values of t and ΔR in the equations, we have the outstanding errors shown in column α of the following table :—

α	β	γ
m	m	m
$-23\cdot3$	$-30\cdot4$	$-27\cdot2$
$+12\cdot3$	$+5\cdot2$	$+7\cdot8$
$+3\cdot7$	$-3\cdot5$	$-2\cdot1$
$+36\cdot2$	$+29\cdot0$	$+29\cdot8$
$+8\cdot5$	$+1\cdot0$	$-0\cdot9$
$-59\cdot7$		
$+5\cdot8$	$-1\cdot9$	$-7\cdot4$
$+34\cdot5$	$+26\cdot7$	
$-18\cdot0$	$-26\cdot0$	

The large outstanding error for the Louvain observation would seem almost certainly to arise from the time of M. Terby's No. 3 being given 1^{h} too early; the correctness of this surmise is rendered almost certain by the fact that the preceding curved marking in his No. 4, made on the same evening, agrees almost exactly with our selected point, and can only be reconciled with T 3 by assuming an error in the time of the latter to the extent mentioned. Under these circumstances, the Louvain observation was omitted, and the remaining 8 equations being solved again, we obtained

$$t = -25^{m}\cdot35 \pm 10^{m}\cdot31,\ \Delta R = -0^{m}\cdot02527 \pm 0^{m}\cdot05578,$$
$$r = \pm 15^{m}\cdot57.$$

The outstanding errors are given above in column β.

As a further experiment, the Parsonstown observations were used alone ; they gave

$$t = -20^{m}\cdot43 \pm 10^{m}\cdot97,\ \Delta R = -0^{m}\cdot06836 \pm 0^{m}\cdot08553,$$
$$r = \pm 14^{m}\cdot12$$

with the outstanding errors given in column γ.

Figures 10 (March 13) and 14 (April 11) present two features in common, these are the markings in the north part of the equatoreal zone. Taking the centre of the preceding marking, and allowing for aberration and phase, we have the times when it was at the centre of the disk, reckoning from January 0, 1873, $72^{d}\ 9^{h}\ 44^{m}\cdot4$ and $101^{d}\ 7^{h}\ 58^{m}\cdot8$, 70 rotations having intervened. Hence the period is $9^{h}\ 55^{m}\ 3^{s}\cdot8$.

In the same way the preceding end of the following spot occupied the true centre of the planet's disk at $72^{d}\ 10^{h}\ 29^{m}\cdot1$ and $101^{d}\ 8^{h}\ 33^{m}\cdot9$, giving a period of rotation $= 9^{h}\ 54^{m}\ 55^{s}\cdot4$. Taking Schmidt's velocity of rotation of the equator of *Jupiter** at 40211

* *Astronomische Nachrichten*, No. 1973.

Paris feet per second, which agrees exactly with Mädler's period, we have for the proper motions of the spots, + 25·6 Paris feet and + 35·1 ft. per second (or 17 and 23 statute miles per hour), both with a probable error of $\pm$ 13·6 ft. The probable error is obtained from the result already arrived at, that the interval of time between two of our drawings, as deduced from the original sketches, has a probable error of $\pm$ $14^{m}\cdot12$. It is well known that results of far greater accuracy may be obtained by noting the exact time when any particular marking passes the centre of the disk, but the limited range of our instrument almost entirely precludes the use of this method.

In fig. 19 there is a dark marking in the equatoreal belt that may be recognised in M. Terby's No. 6. A complete reduction of these observations gives $31^{d}\ 19^{h}\ 8^{m}\cdot2 = 77$ rotations, for the interval elapsed: whence it follows that the spot had a proper motion of + 53·5 Paris ft. + 12·7 ft. per second, or + 36·9 miles per hour.

Schmidt, in *Ast. Nach.* No. 1973, gives a number of cases of proper motion of spots on *Jupiter*, ranging from 10 ft. to 297 ft. per second.

The aspect of the planet given in fig. 16, February 27, will be found represented with as close accordance as can be expected, considering the difference of the instruments used, in M. Terby's No. 2, which was taken two rotations later. The curved marking called *a* by M. Terby can be immediately identified in the Birr drawing; but it would seem to be hardly possible that it is identical with either of those marked with the same letter in his Nos. 9 and 11, but rather that all three markings are entirely distinct. The *a* of M. Terby's Nos. 2 and 11 are *both* shown in our fig. 16.

The blue spot seen March 22 (fig. 4) does not seem to have been very permanent, unless indeed it is shown near the following limb of fig. 1. It was figured by M. Terby once on March 23, and twice on March 25. Unfortunately this side of the planet has been little observed by us, and M. Terby's drawings were made too close to our's in point of time to allow of any deductions being made.

It may not be uninteresting to remark, that Mr. Knobel's No. 1 and M. Terby's No. 9 were taken at almost the same moment.

The nights, January 22, April 10 and 11, 1873 (Nos. 13, 3 and 14) far exceeded most of the others in point of definition; the last night in particular affording almost uninterrupted views of the most delicate markings. It will be readily understood that the definition with a six-foot aperture is, even on the best nights, more or less fitful; often for minutes together all the finer details of a brilliant object like *Jupiter* are mixed up in inextricable confusion, and it is only at more or less widely separated intervals that the confused image suddenly appears to freeze or crystallise into one of great sharpness. It is only in

such moments that details like the small white clouds in I 3 of No. 16 (not particularly well seen in the chromolithograph), or the small dark streaks shown in Nos. 3, 13 and 14 can be seen at all. It seems probable that the latter markings are closely related to those described by Secchi, in the *Memorie del nuovo Osservatorio del Collegio Romano*, 1852-1855, p. 114, with this difference, that what he describes as "a great multitude of bright lines on the obscure part beneath the principal band," * we have always seen as a number of fine dark lines on a bright ground.

Similar appearances are mentioned by Mr. Webb † as having been described by Schwabe and Jacob, but nothing of the kind is shown in the well-known magnificent drawings by Mr. De La Rue and Professor Piazzi Smyth.

The great lóss of colour sustained by the equatoreal belt within the last two years has been a subject of general remark: its extent will be best seen by comparing the two rough drawings of the opposition of 1872 with the others. The redness of that belt in the autumn of 1870 was such that, according to a naked-eye observation by one of us in September of that year, the general colour of the planet's light was affected by it. This observation was made without a previous knowledge of the fact that *Jupiter's* belts were at all redder than usual.

It is very remarkable that while the southern and equatoreal regions of *Jupiter* during the opposition of 1873 were subject to such great changes, the northern regions, and especially the dark belt 4, remained so long unaltered. There is, however, now an end to this state of things, for, on February 22, 1874, 14^h 37^m G.M.T. the planet was seen without a trace of the northern temperate belt: the equatoreal belt was fawn coloured as in fig. 7.

* ". . . una gran moltitudine di linee chiare nella parte oscura sotto la fascia principale."

† *Celestial Objects for Common Telescopes*, 2nd edition, p. 127.

On Two Ancient Conjunctions of Mars and Jupiter.
By the Rev. Samuel J. Johnson.

As a supplement to the ancient Conjunctions referred to in the *Monthly Notices* for January, two important ones may be found in Street's *Astronomia Carolina* (1661), which I have not found mentioned by any more recent authors, and they, perhaps, merit further examination. Street gives the results obtained from the tables of his day. He states:

"Anno Christi 498. May the 1st day, near 7^h reduced to London, ♂ was seen so conjoined with ♃, that there was no

interval between them. The true longitude of ☉ was $0^s\ 29° 20' 6''$. The mean anomaly of ♃ $11^s\ 17° 42' 32''$, his geocentrick place $4^s\ 18° 31' 34''$, with lat. north $1° 25' 30''$. The mean anomaly of ♂ $2^s\ 3° 7' 47''$, his geocentrick place $4^s\ 18° 32' 28''$, with lat. north, $1° 16' 22''$. The difference of longitude is $0' 54''$, of lat. $9' 8''$, at which small distance to the bare eye they might well seem to have no interval or space between them.

"Anno Christi 1170. September the 13th, at midnight, two of the planets were so conjoined that it appeared as if they had been one and the same star, but they were presently separated. *Gervasii Chronicon.*

"These two planets were ♃ and ♂, being so near together that they seemed as one star, but to some eyes a little distinguished.

"The sidereal longitude of the Sun was by our tables $5^s\ 5° 26' 31''$. The mean anomaly of ♃ $7^s\ 23° 51' 50''$, his geocentric place $1^s\ 19° 16' 3''$, with lat. south $42' 44''$. The mean anomaly of ♂ $7^s\ 27° 13' 49''$, his geocentric place, $1^s\ 19° 8' 55''$, and lat. south $39' 1''$. The difference of longitude is $7' 8'$, of lat. $3' 43''$, and hence the distance of their centres $8'$."

Upton Helions Rectory, Crediton,
1874, *March* 9.

On the relative Magnitudes of the Fifth and Sixth Stars in the Trapezium of Orion. By Thomas Barneby, Esq.

Having noticed the discussion at the Meeting of the Society held on the 9th of January last, concerning the relative sizes of the 5th and 6th Stars in the Trapezium of Orion, and having felt an interest in the appearance of those Stars, ever since reading in an early edition of Herschel's *Outlines of Astronomy*, that to perceive *both* was one of the severest tests which could be applied to a telescope, I venture to think the scrutiny I have made of them, with my 9-inch object-glass by Cooke, which I have been told is the best he ever made, may be acceptable to the Society.

On first turning this telescope on the Trapezium some years ago, I saw the 5th star distinctly, but I could not detect the 6th for a considerable time, which caused me some disappointment; but when I first perceived it, I found it so easy that I almost fancied I must have been looking in the wrong place for it before. I could then see it easily with a micrometer-eyepiece, which had evident marks of use by its former owner, the late Captain Jacob. I afterwards from time to time saw the 5th and 6th stars without difficulty, for a considerable period, but I always considered the 5th as by far the more easily seen.

For the last two or three seasons however (before the present),

I have often looked in vain for the 6th star, and I began to think the object-glass was out of adjustment, or my eyesight impared.

Since the constellation has been near the meridian of my Observatory, at a convenient time of night in the present season, I have occasionally examined the Trapezium carefully, and the following is the result:—

I could not detect the 6th star until the 21st of January last.

On that night I saw the 5th and 6th stars distinctly, but the 5th appeared much the larger and brighter of the two.

On the 4th of February last, I saw both stars again, when decidedly the 6th was the more conspicuous, and apparently much the larger.

On the 8th of February last, I could see the 5th distinctly, but the 6th was invisible to me, and also to my assistant, who has a keen and younger eye.

Now I particularly noticed that the 4th of February, when the 6th star appeared the larger, was the best telescopic night; and it appears to me probable that the 6th is really the larger star, but that partly from its blue colour, and partly from its greater propinquity to its primary, and the irradiation of the primary, it is not to be seen so well as the 5th, except on a superior telescopic night.

Is it possible that the variability in the appearance of the 5th and 6th stars, or either of them, can be occasioned by any *motion of translation in space*, such as that indicated by Mr. Huggins in the neighbouring stars *Rigel* and *Sirius*, and other stars, as well as in Nebulæ?

I judged partly of the night of the 4th of February last by the close companion of ζ *Orionis*, which I have found by experience is not to be seen with a clear disk, detached from its primary except on a good night, and on the 4th of February last it was.

There is a great difference in the apparent disks of stars: for instance, the principal star in σ *Orionis* always appears to me as if a flame were creeping about it, and I would direct attention in this respect to λ *Orionis*, and the pointed little reddish bright star, one of the four which attend it, and to the quiet and very beautiful appearance of ρ 1 and 2 *Orionis* which resemble in miniature 'pulcherrima' ϵ *Bootis*, all beautiful objects. I found also that, on the night of the 4th of February last, my telescope revealed the neighbourhood of λ *Orionis* bespangled with very minute bright stars, like a loose cluster.

I viewed these objects again on the 7th of March instant, when I saw the 6th star as well as the 5th very distinctly, and I considered the 6th the larger.

The night was again telescopically good, although not so good as on the 4th of February last.

I have never seen the additional stars in the Trapezium.

On each of the nights above mentioned the nebulous appearance

in *Orion* was very distinctly seen, with its well-known streamers, some of which remind me of the wreaths of smoke which issue from under an extinguisher when putting out the light of a candle.

Morton House Observatory, Worcester,
1874, *March* 11.

Remarks on the Obituary of the late Rev. Professor Temple Chevallier, B.D., Astronomische Nachrichten, 1968, *and Monthly Notices,* vol. xxxiv. No. 4, pp. 138, 139. By R. C. Carrington, Esq., F.R.S.

In both publications there appears the same paragraph, nearly word for word, although the first is signed John J. Plummer and the last R. J. K. which I take to be R. J. Knight.* I listened with much attention to the reading of the obituary by Mr. Dunkin, and should have risen to contradict the paragraph on the spot if I had heard it, but no, it was not read, and there was not anything in the report to which I could take exception. The paragraph is: "He was the first to institute in England the regular, continuous observation of the Solar Spots, which has since led to important results. The methods he employed in these observations were afterwards adopted by Mr. Carrington (at one time Observer at Durham), who has made a similar series of observations with marked success; and astronomers may perhaps feel disposed to regret that Mr. Chevallier's talents were too much occupied by clerical and professorial work to admit of the full development of his powers in the field of original research."

Now, I hold that Harriot was the "first to institute in England the regular, continuous observations of the Solar Spots," and he observed them from December 8, 1610, to January 18, 1613, as I know from having had his observations in my hands to copy at Petworth House, on September 12, 1857.

Secondly, I have to deny altogether the assertion, twice repeated, that "the methods employed by Professor Chevallier were those afterwards adopted by Mr. Carrington," for I left Durham finally on April 8, 1852, and it is recorded by me in my astrono-

* This may probably be explained on the supposition that Mr. Plummer forwarded to the writer of the obituary notice some notes relating to Professor Chevallier's astronomical work at Durham, and that these notes were also incorporated in the communication sent by Mr. Plummer to the *Astronomische Nachrichten.* We may take this opportunity of remarking that at the January meeting, 1849, Professor Chevallier exhibited a volume containing diagrams and observations, the result of "the regular, continuous observation of the Solar Spots," on which he had been employed some time, and that he expressed his intention of presenting the volume to the Society after he had completed the series of observations. In conformity with this intention, Professor Chevallier presented, on July 2, 1851, his valuable series of observations, bound in two volumes, both of which are now in the Society's Library.—[Editor.]

mical journal that, on October 11, 1853, the idea first occurred to me "whilst out walking at Redhill," of taking advantage of the Sun's circularity in the manner which I afterwards adopted, and explained in the *Monthly Notices*, vol. xiv. p. 153, and vol. xv. p. 174. I quote the following from my published work, Preface, p. 2: "To carry out this plan, it was in the first place necessary to devise a new and more commodious method of observation than any hitherto adopted, and to lay altogether new foundations of method in recording, reducing, comparing, and discussing, for I unhesitatingly say that no observer would for any length of time have followed out any of the modes of observation previously practised."

Note on a Paper by Mr. Stone, "On the Rejection of Discordant Observations." By J. W. L. Glaisher, Esq.

Considering the tone of Mr. Stone's remarks in the *Monthly Notices* for November (vol. xxxiv. pp. 9–15), I felt myself justified in expressing with frankness what I thought with regard to his views and his criticisms on my own.

It was suggested to me that the paper containing these opinions—necessarily somewhat personal—should be withdrawn, and I was advised that the matter rest where it was, without further comment, a course which is, perhaps, the better. I have stated what my belief is in the *Monthly Notices* for April 1873 (vol. xxxiii. pp. 391–402); and have now only to ask the reader not to assume that I hold, because I offer no reply, all the opinions attributed to me by Mr. Stone, *e.g.* in reference to the investigation on pp. 14 and 15. I never held that the h of every observation was *à priori* equally likely to have any value from 0 to ∞, as I took the usual result as a first approximation. I will also ask any one who wishes to form an opinion on the subject in question to read Mr. Stone's paper in the *Supplementary Number* (vol. xxxiii. pp. 570–572), as well as that already referred to.

Second Paper on the probable Variability of some of the Red Stars in Schjellerup's List (Astronomische Nachrichten, No. 1591). By J. Birmingham, Esq.

(*Communicated by Mr. Dunkin.*)

At the last December meeting of the Society I had the honour of laying before it an account of the disappearance of No. 252 in the above Catalogue; and, while noticing certain pos-

sible grounds of doubt respecting the reality of the original discovery of that star by Sir John Herschel, I argued against the probability of his making the double mistake of regarding Bessel's adjacent red star as a new one, and also assigning to it a wrong position. It was, therefore, with no little satisfaction that I saw by Mr. Chambers's paper read at the January meeting of the Society, that he observed the star in 1870, together with the neighbouring star, 251, and I have lately found that 252 was seen and spectroscopically examined by Secchi on July 17, 1868; so that it seems abundantly proved that 251 and 252 were distinct objects, and that the latter has recently disappeared.*

I would now direct attention to the probable variability of other stars in Schjellerup's list, which are now convenient for observation.

I first searched for No. 90, on February 3, 1873, and failed to find a red star in, or near its position. I was equally unsuccessful on February 6 and 8, on September 23, and December 26; and on December 28 I wrote to Messrs. Dunkin and Lynn, of the Royal Observatory, requesting that it might be looked for. This was accordingly done, and Mr. Downing, who made the observation with the Transit-circle, found, on January 3, that the exact position of 90 was occupied by a 7-magnitude colourless star without even a tinge of red. It was afterwards identified as one of Flamsteed's stars, 44 *Camelopardali*. This star, which I knew, appeared to me rather less than the 7th magnitude and of a bluish white colour. Schjellerup notes it as one of the stars in the Dorpat Catalogue, where it is rated at the 7th magnitude and marked *rubra*; so that it seems to be a variable at least in colour, if not in magnitude.

No. 101 is taken by Schjellerup from a note by Piazzi, who describes it as red, without stating the magnitude, and rather roughly gives its position as preceding by about 2′ another of his stars—No. 187—on the same parallel. I failed to find the red star on February 6, 1873, and was informed by Mr. Knott, to whom I communicated my failure, that he was equally unsuccessful on two previous occasions—on June 25 and August 3, 1866. However, on January 13 of the present year, I found a star in the position of 101, as well as I could identify it in a hurried observation. The passage of clouds did not permit me to examine it very closely, but it appeared about the 8·5 magnitude, and slightly tinged with red. I have not observed it since then; but I would suggest that it should be closely watched in

* Professor Argelander remarks (*Astr. Nach.*, No. 1977) that this star was looked for at the Bonn Observatory on February 26, 1858, with the heliometer, and on November 18, 1859, with the meridian-circle, but on neither occasion was it visible. He is of opinion that there is some error in Sir John Herschel's observations; and Professor Schönfeld expresses the same view, suggesting that though Sir John speaks of "two observations," they may not have both included determinations of place. On the other hand, we have the observations of Mr. Chambers detailed in the January number of the *Monthly Notices*, p. 104. The point appears an interesting one.—[Editor.]

its present excellent position for observation, when its very probable variability might be determined.

In No. 74 I have remarked differences within the limit of one magnitude, with considerable changes of colour: Schjellerup thus notes it—*Bessel, roth, Cape Obs., vivid red,* 8.

My observations of it were as follow:—

1871, March 8.	Pale red, 8.
1872, Feb. 10.	Orange, 8.
1873, Feb. 2.	Orange, 7·5 to 8. A *comes* 9·5; intense blue; 285°.
1873, Dec. 20.	Fine orange red, 7.
1874, March 6.	Fine orange red, 7.

Recently, the angular distance of the comes was found to be 84″; position 288°.

No. 77, described by Schjellerup as *roth,* 7·7, seems less than 8, with the palest possible red tinge.

It may not be out of place here to refer to a very remarkable orange star which I found when looking for 77. On February 2, 1873, it appeared about the 7th magnitude, with a comes of the 11th; position 95°. On December 26 the large star seemed of the 6th magnitude. On February 3 it looked nearer the 5th, and on March 8, it appeared of the 6th magnitude again. Its roughly approximate place by equatoreal is R.A., $6^h\ 14^m\ 40^s$; Decl. − 2° 54′ 36″. Its small apparent changes may, indeed, be due to errors of estimation; but I consider it worth the attention of observers, as it is strikingly superior in depth of colour to very many of the stars in Schjellerup's Catalogue; and the fact that it was missed by previous observers of red and orange stars is strongly suggestive of variability. From recent measures, I find that the angular distance of the comes is 100″, and the position 99°.

No. 63, unless I mistake its identity, presents an instance of complete change of colour. It appears as one of Schjellerup's own stars in the list, where it is marked "*roth,*" but to me it always shows a fine blue tint, while 51 *Orionis*, near it, is deeply orange.

No. 152, which is described in the Catalogue as a red star of 8·5 magnitude, has been observed by me on several occasions as a fine orange star of the 6th magnitude.

No. 280 furnishes an undoubted instance of change. It is catalogued as one of Argelander's stars of the 6th magnitude, and followed by a blue star on the same parallel. On April 18, 1873, I noted it as a good red, but no more than 7·5, and estimated the blue star at the 9th magnitude. On January 15, 1874, the red seemed only of the 8·5 magnitude, and it was considered even smaller by the Rev. Mr. Webb, on the 12th. On February 17, I thought the star had risen to the 7th magnitude, with a high colour; and, in an unfavourable observation

near lower transit, on February 27, I estimated it at only the 8th magnitude. The small blue star near it seems always about the 9th magnitude, and affords a convenient object for comparison. I have just been informed that Secchi saw 280 of about the 8th magnitude, and suggested its variability, which may be regarded as certain; and it now remains to discover the period.

Notes on the Zodiacal Light. By the Rev. Samuel J. Johnson.

What Humboldt speaks of as the "mild pyramidally shaped zodiacal light, very visible to the unassisted eye," has been displayed here this winter with far more distinctness than I have noticed since February 21, 1870, when I witnessed a vivid appearance of the phenomenon from Lytham on the Lancashire coast.

It was conspicuous, amongst other nights, on February 8, when the impression that Tycho mistook the Light for the "abnormal vernal evening twilight," appeared at first sight almost pardonable.

February 16. Sky clear for a brief interval about 8 P.M. The conical figure very fairly defined, except at the apex, where the curvature was somewhat difficult to make out. *Mars* situated nearly on the axis; about which point, the Light seemed equal in brightness to that portion of the *Milky Way* that passes through *Cassiopeia*. Nearer the horizon, the intensity was decidedly greater, ν *Ceti* appeared just outside the cone of light; the head of *Aries* faintly involved in it. It could be traced, though with difficulty, 3° or 4° above the *Pleiades*.

February 18. Could readily be followed before the Moon set. Boundaries, so far as could be made out, the outline being indistinct, on the left θ, ν, μ *Ceti*, λ *Tauri*, then passing round the *Pleiades*, about which region the figure was very faint. On the other side 41 and α *Arietis* were just outside it. From this point the boundary line would have to be drawn through the 5th mag. stars ψ and τ *Pegasi* to the horizon. Clear extent at the base 30° to 35°. Not quite so brilliant as on the 16th. I fancied a slight reddish tinge in the brighter portions.

February 19. The Moon did not set till $9^h\ 14^m$, when the more conspicuous parts had descended below the horizon.

An interval of moonlight nights followed.

March 6. The Zodiacal Light again conspicuous. In extent and general features unaltered; in intensity scarcely so great. The clearest defined portion lay between ν *Ceti* and γ *Arietis*: at lower altitudes, the Light, although brighter, appeared very much diffused. *Mars* about 5° left of the axis.

March 7. With regard to the earliest visibility of the Light,

it was not noticeable till 15^m after stars of the brightness of γ *Arietis* had shone out, and not quite so soon as the *Milky Way* at equal altitudes. Its whiteness more dusky than the latter. At an altitude of about 20° η and α *Piscium* (the latter just within the boundary) were somewhat dimmed by its intensity.

Upton Helions Rectory, Crediton,
1874, *March* 9.

Note on the Zodiacal Light. By E. B. Knobel, Esq.

I would beg to direct attention to the unusual brilliancy of the Zodiacal Light this winter.

It is stated in text-books that the Zodiacal Light is only visible in the evenings, in this country, in the spring months; and I have repeatedly observed it in March and April, but have never seen it more brilliant than in January and February of this year.

On two clear evenings in the first week in January, on January 17, at 6 45 P.M., and lastly on February 8, at 7 P.M., it appeared as an elongated luminous cone, the apex of which on January 17 extended nearly to the star γ *Arietis*, and on February 8 the apex just enclosed η *Piscium*.

It appeared nearly as bright as the *Milky Way*, and sufficiently bright to attract the attention of a casual observer.

I should mention that my situation is quite away from the town, and sufficiently high to be above the mists of the valley.

Stapenhill, Burton-on-Trent,
1874, *Feb.* 11.

On the Structure of the Solar Photosphere. By S. P. Langley, Esq.

(*Communicated by Mr. Lockyer.*)

During some years past the spectroscopic investigation of the solar surface has furnished new and valuable results so continuously, as to engross the attention of observers almost to the exclusion of older methods. I have been led, however, to think, that much of interest remains to be reached by direct telescopic scrutiny of the Sun, and that among the facts thus to be gathered are some concerning which the spectroscope cannot inform us, and some which will aid us to interpret its indications.

It will be remembered that an interesting controversy arose

out of the announcement of Mr. Nasmyth's "willow-leaf" structure; and that, after eliciting valuable communications from Messrs. Dawes, Lockyer, and others, it may be said to have closed with a paper by Mr. Huggins, which appeared in the *Monthly Notices* for May 1866. Since this, with the exception of some observations of Padre Secchi, little of essential importance has been added to our knowledge of the intimate structure of the photosphere. As the nomenclature of this subject is in complete confusion, it is necessary to call attention to the fact, that I here mean by "rice-grain" exactly what Mr. Huggins denotes by "granule,' (I quote him here from memory), and that I reserve the latter word for a restricted and distinct use. The word "rice-grain" is descriptive of the appearance of certain components of solar clouds, but has no special value as a simile, save for what is seen with moderate apertures;* such as were employed by Messrs. Stone and Dunkin, who first used the word. By "rice-grains" are here meant certain universal constituents of solar clouds, visible only in fine definition; presenting a precise shape as compared with those larger but vaguer cloud-forms or mottlings which are visible on the photosphere with lower powers; of an irregular outline, frequently rudely oval; whose average dimension of from 1″ to 2″ is yet sometimes fallen short of, sometimes exceeded; which to ordinarily careful observation seem in close juxtaposition, and separated by narrow intervals which are not uniformly dark; the rice-grains themselves being of vivid, but unequal brilliancy, and in the language of Sir John Herschel, "the immediate sources of the solar light and heat."

With an object-glass of thirteen inches, possessing great perfection of figure, and which has been employed with undiminished aperture by aid of the polarising eye-piece, I was led to notice that the rice-grains were by no means in such close juxtaposition as at first appears; the large aperture, by reducing irradiation, helping to make it more easily visible, that these bodies, though the chief sources of the solar light, occupied but the smaller portion of the solar surface.

In measuring them by the filar micrometer, and in reckoning their number (by the aid of a reticule kindly ruled for me in minute squares on thin glass for this purpose, by Mr. Rogers), I was led to notice that my estimates of both size and numbers varied with the magnifying powers employed, in a way which seemed unaccountable on the supposition that rice-grains were individual things of approximately uniform size. By taking advantage of the brief and rare intervals of definition, which admit the use of the high powers of such a telescope, I found the cause in the resolution of the rice-grains into an order of minuter components, hitherto scarcely observed. These components I here

* Mr. Stone observed the "rice-grains" with the Great Equatoreal at the Royal Observatory, the clear aperture of which is 12·8 inches.—[EDITOR.]

term *granules* (a word used by others as a synonym for "rice-grains," and to which a distinct and restricted significance is, it should be noticed, here attached). They are very minute bodies present over the whole solar surface, faintly discernible in the faculæ, and in the penumbræ of the spots are extended into long filaments, whose aggregation forms the "thatch-straws" of Mr. Dawes, as the aggregation of granules forms the rice-grain. The latter term should be employed hereafter, I think only so far as it may be necessary to recognise a tendency of these granules to unite in clusters of approximately uniform size. The granules are occasionally seen singly, more frequently united in clusters of from two or three to ten or more ("rice grains"), and by their degree of juxtaposition, and perhaps by their partial superposition, form the inequalities of brilliancy of the rice-grain noted by Mr. Huggins, and account for the irregular outline of the latter, which he has already remarked upon. With the largest apertures and powers not only then do these brilliant bodies appear smaller, but from their apparent area is to be taken the minute dark spaces which it now appears intervene between their own component parts.

It hence became a matter of interest to determine approximately the average number of granules to a given area, and their size, since it was increasingly evident that the part of the solar surface which is principally concerned in sending us light is much smaller than has been supposed.

The general background on which the granules appear is grey by comparison, not uniform in shade but faintly mottled, and its light is very probably also due to the presence of granules, partly dissipated or concealed.

We are not now concerned with this further than to remark that, if this be so, it does not affect the following estimates of the brilliant area, which rest upon the fact that though granules and mottled interspaces here and there merge into one another, there is yet between these ultimate constituents of solar clouds and their background, a distinction so great that we are warranted (for the purpose at least of a first classification), in treating the solar surface as though it were composed of uniformly bright granules, divided by uniformly grey intervals in which the light is certainly so inconsiderable that the granules appear brilliantly irradiant.

In our measurements, let us first remark, that owing to the vivid irradiation of these bodies, a conclusion as to their real size from their apparent angular dimension would be nearly as unwarrantable as a similar estimate would be in the case of a bright fixed star. To evade this difficulty altogether seems impossible, yet I believe the course adopted fixes an upper limit for their apparent size. It has been remarked that the granules are continuous with certain long filaments in the penumbræ; and these filaments, though often much contorted, are in the normal type of penumbra radially disposed, and closer as they converge

toward the centre. By repeated measurements with the filar micrometer, and by other means, it was found that the distance of these filaments varied widely, but that where they lay closest their centres were certainly less than 0″·6 apart. As it may be safely assumed that two bright lines separated by an interval of less than 0″·3 would appear as one in the instrument and with the power employed, it appears that the interval between the filaments observed at least equals their width, which cannot in this case be more than three-tenths of a second of arc, and which, not improbably is much less. But filament and granule are apparently different aspects of the same thing, the granules being the upper end of filaments disposed, in this view (which is partly Mr. Dawes's) in a general sense vertically over the Sun.

The granule being certainly not materially larger than the filament at its extremity, we cannot consider it as over 0″·3 or 0″·4 in diameter. I feel increased confidence in the result of these difficult measurements, from the fact that Secchi, who is the only observer who appears to have seen them, speaks of minute objects he terms *grains*, as covering the whole solar surface, and whose diameter he estimates at from a third to a quarter of a second of arc. These, are doubtless, identical with the objects here termed granules, though they are treated by Secchi as identical with the "rice-grains" of Stone, and the "willow-leaves" of Nasmyth, with neither of which will their size permit us to confound them. Having the size of the granule at its upper limit, their number is obtained by the aid of the Rogers-reticule and by actual count. This is a work of extreme difficulty. I find that if the granules were uniformly distributed we should have rather less than one to each second of arc; but I should here state, that I have reason to believe it probable that neither granules or filaments are "entities" in any other sense than "rice-grains" are. Filaments of immeasurable fineness are at times seen projected on the umbræ, looking, collectively, like carded wool, which are intimately connected with those measured; and though it would not be right to ignore the fact of a tendency to certain more or less definite groupings, which have a real existence, and to which we give here such names as "granules" or "rice-grains," according to their order of magnitude, we must not be misled by our own nomenclature to claim for any of these an objective reality in the sense at one time attributed to the "willow-leaves." There is no improbability then that the division will be hereafter carried further; but it is essential to note that as the increase of the number will imply a proportionate diminution of size, we have, if our present estimates of the relation between size and number be correct, already data which will enable us to fix an upper limit of the ratio of the proper illuminating surface to that of the whole photosphere, which cannot be disturbed by subsequent minuter discrimination. I do not feel that the data given are exempt from considerable possible error, owing to the great rarity of the occasions when they can be gathered or verified; but after making

every reasonable allowance for this, we cannot reach the result of our computation without surprise, for it leads inevitably to the conclusion that the Sun's light comes chiefly from an inconsiderable portion of its surface, and I cannot myself estimate this portion at more than one-fifth of the whole.

I have been led to think that the study of the forms assumed by the filaments will give information of interest as to the character and direction of the currents in the photosphere, whose existence Mr. Lockyer and others have already demonstrated by the spectroscope. These may be studied to most advantage in the penumbræ. The darker shade, around the outer penumbral edge, is, I observe, ordinarily only that of the grey matter everywhere on the surface, serving as a background for the granules; and the evidence from a prolonged study of the minute structure here, seems to show that this edge is formed by rupture, by a stress that is, acting laterally and downwards, and not by a deposition of the constituents of ascending currents.

These filaments are not only more brilliant at the umbral edge (as if their extremities were curved upward, and less obscured, where partly elevated above a darker supervening medium), but this tendency may be traced in good definition, all over the penumbra, in which they have a certain tendency to unite in narrow sheets or plates, which, superposed, form the fascicles called "thatch-straws" by Mr. Dawes. In the umbra, the detached bright points, observed by Mr. Lockyer and others, are found to be tips of filaments thus raised, the part of the filament extending under the umbral shade being frequently, by proper precaution, dimly, but unequivocally traceable through it, and found continuous with a portion of the penumbra. By the aid of the polarizing eye-piece, and by excluding as far as possible extraneous light, the whole umbra is seen at times to be nearly or wholly made up of sunken banks of the filaments; the umbral structure being quite complex, and presenting the appearance of a submerged penumbra, and the nuclei of Mr. Dawes, being only deeper portions of its shade.

By means, for the detailed description of which there is not here space, I have isolated one of these nuclei from the surrounding umbra. Mr. Dawes terms the nucleus *intensely black* (the italics are his), but thus viewed it is not "black," not even dark, except in a relative sense, but brilliant with a violet-purple light. I regret that I have been unable to obtain, as yet, photometric measurements of the luminosity, but when seen alone it might fairly be described as dazzlingly vivid, the appearance being like that of a violet star, and very striking. The observation is one which may have been already made; yet I have not seen any determination of the light of the nucleus as distinguished from the umbra, and its interest in connection with the disputed question of the transparency of the Sun's interior induces me to give it a place here.

I have never been able to satisfy myself by direct telescopic

scrutiny, whether these "banks" in the umbra have an invariable direction of motion, whether of ascent or descent. Nearly all my observations have tended to confirm me in the impression that they moved as if under the direction of a downward current; yet in one instance I have seen the contrary, and in this case the evidence of ascent seemed unequivocal. Mr. Lockyer (*Monthly Notices*, June 1865) has noticed in one instance the fact of the existence of superposed currents moving in different directions. I think this nearly isolated observation may be greatly extended; and I can, from what I have repeatedly seen, entertain scarcely more doubt of the frequent superposition of approximately horizontal currents of the different strata of solar clouds than of our own.

What may be almost called the typical form of solar currents, the cyclonic, has been left unmentioned. Of the various kinds of this as traceable by the disposition of the flexible filaments, I have made a very considerable number of sketches which were in every case finished at the telescope, and though inartistically, yet, I believe, not unfaithfully represent the forms seen there. Any complete discussion of these would be impossible here, but it may be stated generally—

That the normal type of cyclone contemplated by M. Faye as embracing the whole spot in a common movement of rotation is extremely rare.

That the evidence of cyclonic action is nevertheless everywhere present in the spots. The radius of curvature of the filaments diminishes rapidly to the centre of the whirls, which are very numerous in large spots; often completely independent when in apparent juxtaposition, or even apparently in part superposed, and sometimes rotating in different directions.

That the axes of these whirls are not always vertical, but are occasionally met with inclined at all angles.

That the filaments in incipient, small, or growing spots are less frequently seen moulded by any uniform cyclonic action than in forms whose disposition is nearly that seen in processes of crystallisation, or gemmation.

Padre Secchi almost alone has given some illustrations of these "crystalline" forms, and it is to be wished he had given more. I have myself seen some so unlike anything we should be prepared to expect from the ordinary published illustrations, that I should be glad to have the independent evidence of so eminent an observer to refer to in making them public. I can only say here, and in the absence of special illustrations, that this crystalline appearance is very remarkable not only in itself, but from its contrast with the flexible forms the filaments ordinarily assume, and that the most peculiar types are confined to large spots, such as at the present time rarely present themselves.

Both in their points of resemblance to, and distinction from ours, however, the solar clouds are of interest to the terrestrial

meteorologist; but many more observers are wanted in this direction than we have at present, and it would be especially desirable if photography could do for the details of spots what it has already done for the laws of their movement. Our own atmosphere is the great obstacle, and a more serious one in observations by day than by night, as weeks may pass without bringing an hour of definition suitable for the best use of the very large apertures indispensable for some of the observations which have been detailed, and their demand upon the time of the professional astronomer having other duties is frequently too great to be met. I should hardly have felt justified in commencing observations of this kind had I known the assiduous attention they require in awaiting what are in our climate the rare moments when they can best be made. I shall feel less regret for the disproportionate time thus spent, if the present communication should be at all instrumental in directing the attention of those private observers now possessing powerful instruments of a class comparable with that used to a most interesting field of research, as yet scarcely occupied, and in which their labours may be instrumental in laying the foundations of a future science of solar meteorology.

Allegheny Observatory,
Allegheny, Pennsylvania, 1874, *January* 30.

On the Approximate Calculation of the Times of Solar Eclipses.
By J. N. Lewis, Esq.

(*Extract from a Letter to Mr. J. W. L. Glaisher.*)]

In the *Monthly Notices*, Vol. xxxii. p. 332, is an article by the Rev. S. J. Johnson, "On Future Solar Eclipses." In calculating these he makes use, for brevity, of the Tables contained in the eighth edition of the *Encyclopædia Britannica*, and he says he finds only one instance in which they are more than nine minutes out in the time of greatest obscuration. This recalls to mind that I prepared, a few years ago, a set of Tables of still greater brevity than those in the *Britannica*, and, according to the testimony of Mr. Johnson, more accurate also. As to this latter point, some results are given in the following Table. The Table of Epochs was prepared from the elements of the Moon's orbit given by Hansen (*Tables de la Lune*) and of the Sun's orbit by Le Verrier (*Annales de l'Observatoire de Paris*, tome iv.) taking into account the sécular equations; and the equations to reduce the mean to the true syzygy were computed from the formula in Burckhardt's *Tables de la Lune* (Paris, 1812) p. 87, two small Tables being added for the purpose of taking into account the "Reduction," and the change in the velocity of the Moon's motion between the mean and the true syzygy; also for the Moon's latitude,

parallax, &c. Very brief Solar Tables are also added. These Tables were intended to be used, in connection with a set of diagrams, or graphical constructions, for making a rapid examination of all the recorded ancient eclipses. They will give a pretty satisfactory representation of the circumstances of any eclipse from B.C. 720 to A.D. 2000, and thus save a vast amount of calculation that might otherwise be wasted upon eclipses that would turn out to be of no importance. They would thus serve as a criterion to indicate or point out the cases where exact calculations might be resorted to, without the risk of losing the labour and time expended. Among others, I intended to apply them to an examination of the Chinese eclipses, lists of which are given by Mr. Williams in Vol. xxiv of the *Monthly Notices*; but a press of other business has caused me to lay aside the whole matter until I shall have more leisure.

I send you the following comparisons. The results from the approximate Tables were, in each case, obtained by a few minutes of calculation. The numbers in the columns headed "True time" and "True latitude" are the results derived from extended Tables :—

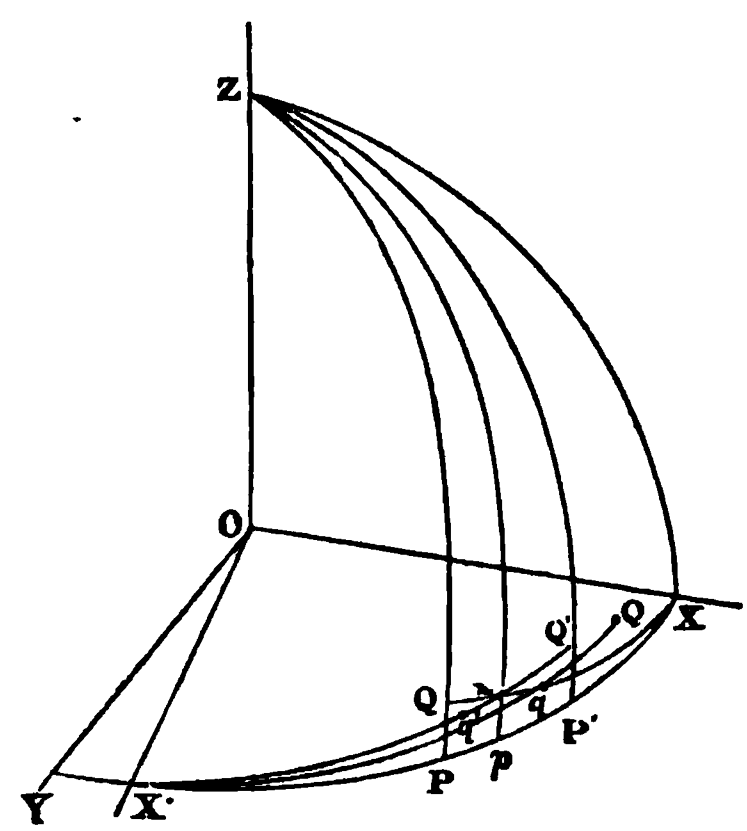
Approx. Time of Syzygy.				True Time.			Discordance.	Moon's Latitude. Approx.	True.	Discordance.
	d	h	m	d	h	m	m	′	′	′
1831	Feb. 12	5	14	12	5	13	+1	+42		
1832	July 27	2	1	27	2	2	−1	+ 4	+ 3	+1
1834	Nov. 30	6	45½	30	6	47·7	−2	+53	+51	+2
1836	May 15	2	7	15	2	7	0	+26	+26	0
1838	Mar. 25	9	45	25	9	45	0	−47	−46	−1
1838	Sept. 18	8	45	18	8	45	0	+47	+48	−1
1842	July 7	19	0	7	19	1	−1	+28	+28	0
1844	Dec. 9	8	12	9	8	13	−1	+72	+72	0
1846	Apr. 25	4	48	25	4	48	0	+11	+12	−1
1850	Aug. 7	9	34	7	9	33½	+ ½	0	+ 1	−1
1851	July 28	2	41	28	2	40½	+ ½	+45	+46	−1
1854	May 26	8	46	26	8	47	−1	+23	+21½	+1½
1858	Mar. 15	0	13	15	0	12	+1	+38	+38	0
1860	July 18	21	19	18	2	19	0	+34	+33	+1
1865	Oct. 19	4	27	19	4	26	+1	+30	+29	+1
1868	Feb. 23	2	19	23	2	20	−1	+ 5	+ 4	+1
1869	Aug. 7	10	6	7	10	7	−1	+41	+42	−1
1871	Dec. 11	16	1	11	16	1½	− ½	+11	+11	0
1878	July 29	9	39	29	9	39	0	+38	+37	+1
1900	May 28	2	48	28	2	48	0	+23	+23	0
1905	Aug. 30	1	11	30	1	11	0	+35	+35	0

The parallaxes, hourly motions, &c., are given with a like degree of approximation.

Mount Vernon, Ohio, U.S.,
1874, *March* 2.

Note on the Curvature of Lines in the Dispersion Spectrum, and the Method of Correcting it. By W. H. M. Christie, Esq.

When a spectrum is formed in the ordinary way with a spectroscope, the lines are curved, and no adjustment of the prisms will get rid of this serious defect. This point has, I believe, not been investigated, though there is no great difficulty in the problem, in the simple form it assumes when the pencil, passing through the prism, consists of parallel rays—a condition which ought always to be satisfied in a properly adjusted spectroscope. It will in this case be sufficient to consider the directions of the rays, in order to show that the image of the slit, after refraction through a prism, will be curved.



Taking the plane of $y\,z$ for the face of the prism on which the rays are incident, O Z for the refracting edge, and O X the normal to the face, let P O, Q O be the directions of pencils from the centre and one extremity of the slit respectively, p O, q O the directions of the corresponding refracted pencils, n the intersection of the arcs Z p and Q X, and let $\angle$ P X Q $= a$,

Then

$$\frac{\sin QX}{\sin qX} = \frac{\sin PX}{\sin pX} = \mu,$$

and

$$\frac{\cot nX}{\cot QX} = \frac{\cos a \cdot \cot pX}{\cos a \cdot \cot PX} = \frac{\cot pX}{\cot PX}$$

by the right-angled spherical triangles P Q X, $p\,q$ X;

$$\therefore \frac{\cos nX}{\cos QX} \cdot \frac{\sin QX}{\sin nX} = \frac{\cos pX}{\cos PX} \cdot \frac{\sin PX}{\sin pX} = \frac{\cos pX}{\cos PX} \cdot \frac{\sin QX}{\sin qX}$$

$$\therefore \sin nX = \frac{\cos PX}{\cos pX} \cdot \frac{\cos nX}{\cos QX} \cdot \sin qX;$$

but

$$\cos nX = \cos pn \cdot \cos pX$$

and

$$\cos QX = \cos PQ \cdot \cos PX$$

$$\therefore \sin nX = \frac{\cos pn}{\cos PQ} \cdot \sin qX > \sin qX,$$

or,

$$nX > qX.$$

Therefore, the image of a straight line after the first refraction is concave towards the normal to the first face. The amount of this curvature may readily be found, as follows:

From the last equation—

$$\frac{\sin nX - \sin qX}{\sin nX + \sin qX} = \frac{\cos pn - \cos PQ}{\cos pn + \cos PQ},$$

or,

$$\frac{\tan \frac{1}{2}(nX - qX)}{\tan \frac{1}{2}(nX + qX)} = \tan \tfrac{1}{2}(PQ - pn) \cdot \tan \tfrac{1}{2}(PQ + pn),$$

which gives, since $nX - qX$, PQ and pn are small quantities,

$$nX - qX = \tfrac{1}{2}(PQ^2 - pn^2) \cdot \tan qX \text{ nearly};$$

but

$$\sin PQ = \sin a \sin QX,$$

$$\sin pn = \sin a \sin nX = \sin a \sin qX \text{ nearly}$$

$$\therefore \frac{pn}{PQ} = \frac{\sin qX}{\sin QX} = \frac{1}{\mu};$$

Hence

$$nX - qX = \frac{\mu^2 - 1}{2\mu^2} \cdot PQ^2 \cdot \tan pX \text{ nearly}.$$

For the refraction, on emergence, let OX' be normal to the second face of the prism, $P'O$ the direction of the emergent pencil (incident in the direction PO), $X'nQ'$ an arc of a great circle: then, if we suppose the direction of the emergent pencil reversed, the ray OQ' would be refracted in the direction Oq' (q' being a point on the arc nX'); and therefore the pencil which passes through the prism in the direction Oq will emerge in the direction $OQ_{,}$, $X'qQ_{,}$ being an arc of a great circle, and $Q_{,}X' > Q'X'$; therefore, the image on emergence will be concave towards the normal to the surface of incidence and its curvature, after refraction through the prism, will be—

$$\frac{\mu^2 - 1}{2\mu^2} \cdot PQ^2 (\tan pX + \tan pX'),$$

where pX' may be negative, but is always numerically less than pX, since the deviation is always from the edge of the prism.

This expression will be a minimum when the pencil passes through the prism with minimum deviation, for let $pX = \phi'$ and ι = angle of prism,

$$\tan pX + \tan pX' = \tan \phi' + \tan(\iota - \phi') = \frac{\sin \iota}{\cos \phi' \cos(\iota - \phi')}$$

$$= \frac{2 \sin \iota}{\cos \iota + \cos(2\phi' - \iota)},$$

which is a minimum when $2\phi' = \iota$.

Hence the curvature cannot be got rid of by any adjustment of the prism, and increases with every prism used.

But if, after passing through the train of prisms, the rays be reflected directly back again by a plane mirror, so as to pass through the collimator, and form an image slightly on one side of the slit (which image may be viewed by means of a diagonal prism), the course of the pencil being reversed, the curvature will be corrected by the second passage through the prisms, and the dispersion will be the same as if the pencil had passed through a train, formed by the prisms themselves and their reflections in the plane mirror.

This is done in the Spectroscope now being made for the Royal Observatory, and the lines are perfectly straight.

When the ordinary right-angled prism is used at the end of the train, there is an inversion with regard to up and down which prevents any compensation of this kind from operating.

Blackheath,
1874, *March* 10.

On a Method of drawing, by continued Motion, a very close Approximation to the Parabola, proposed with a view to its possible Application in figuring Reflectors. By F. C. Penrose, Esq.

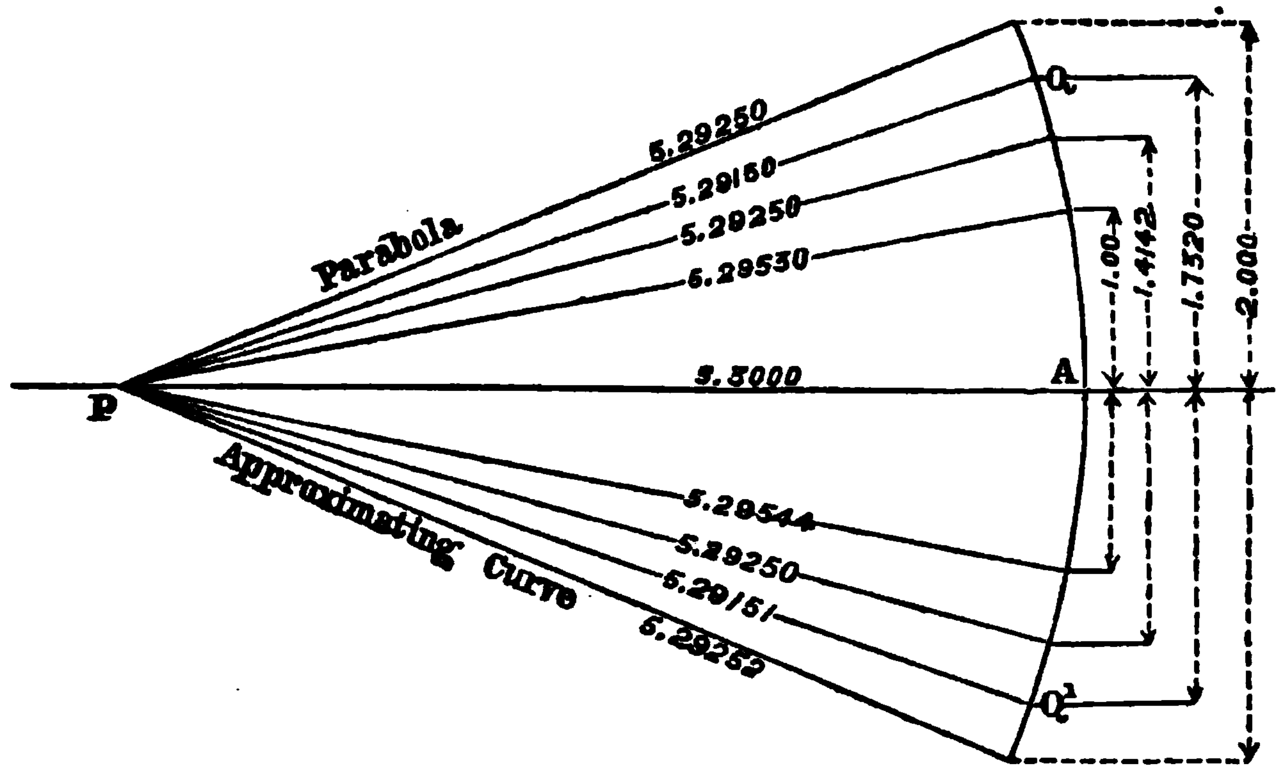
Professor Sylvester, in his Lecture at the Royal Institution in January, showed how two of Peaucellier's parallel motion cells might be combined so as to draw the Conic Sections.

This instrument, however, when arranged so as to draw the Parabola (which it does with theoretical exactness), owing to its working at a mechanical disadvantage, could hardly be made available in practice where steadiness is of the utmost importance.

Another application of the instrument, of which a working model is produced, moves remarkably steadily, and although the curve drawn is not the Parabola in theory—for it is a line of the third order—yet, for a sufficient arc for the majority of optical purposes, may be made to coincide with it so accurately as not to vary from it more than—say, a three hundred-thousandth part of the aperture in a mirror intended for a telescope. I am not speaking of errors of execution, but of theoretic divergence, and various contrivances could be applied to make the execution very exact.

The case which I give in Fig. 1 supposes a reflector of very short focus, not exceeding four times the aperture.

Fig. 3.



Differences of Radius Vector.

+ ·00008

+ ·00001

± ·0

± ·00014

± ·0

The two curves are put together at the vertex, and also are adjusted at Q or Q¹, where they have a common normal, the radii of curvature having also very small difference.

The abscissæ at the places compared are 0·1, 0·2, 0·3, 0·4.

The polar equation to the curve is $\frac{m+\mu}{m}$ $c \cos + \theta \frac{m}{c} \sec \theta = \zeta$ where m and μ are the difference of the squares of the lengths of the links forming the cells of the instrument, and c is the diameter of the circle upon which it is made to revolve.

Wimbledon,
1874, *March* 12.

Notes on some Spectroscopic Observations of Sirius, γ Argûs, &c. By E. H. Pringle, Esq.

(*Communicated by Capt. J. Herschel, R.E.*)

The following observations were made at Mangalore, South Canara, with a Spectroscope of two prisms of dense flint glass, with refracting angle of 60°, attached to a 6½-inch silvered-glass speculum telescope.

Most of the observations have been made after removing one

of the prisms, and replacing the short slit by one of greater length, for it was found impossible to keep the image of a star in view with a small field owing to the telescope having altazimuth motion. The measurements, which must only be regarded as approximate, were taken by the angular motion of the small observing telescope.

The eye-piece generally used has a power of about 11.

I first examined *Sirius* on the 11th inst., removing the slit and using a cylindrical lens, and noticed a band in the violet, which I do not find chronicled. Subsequently on the 18th, a night of remarkable clearness, a still more refrangible line was visible, and beyond this again an extent of violet light. On the 19th, and two succeeding nights, after fitting a new slit to the instrument, measurements of these lines were obtained.

A noticeable point in this star's spectrum is the brilliance—comparatively speaking—of the violet light. It argues an absence of the numerous lines and bands which mar this part of the solar spectrum.

The lines X and Y appear very broad, much more so than the hydrogen lines in the star. A cylindrical lens was occasionally used; but for the purpose of measuring I found keeping the observing telescope very slightly out of focus enabled the lines to be distinctly seen, whilst there was less loss of light. Instrumental parallax was guarded against as far as possible, but as before noticed the measurements are only approximate.

γ Argûs. This is the star whose bright line spectrum was observed in December 1871, by Professor Respighi. In January 1872, ignorant of the above discovery, I accidentally hit upon this star, and examined its spectrum with a small direct vision spectroscope, attached to an achromatic of 3-inches aperture.

The three principal bright lines, two in the yellow and one in the blue, were very distinct; and I suspected a fourth still more refrangible than the yellow lines.

I have now re-examined the star, and find the suspected line has an existence. There is, moreover, a fifth line or band near to, and less refrangible than the blue line. This band seems to be defined on the blue, but to fade off gradually towards the red.

I suspect either bright lines or absorption bands in the orange, from the unequal brightness of this part of the spectrum.

Zodiacal Light. The many observations I have taken of this phenomenon have failed to elicit more than a faint diffuse spectrum. I have had—and fear that this season I shall have—no opportunity of examining the Light from the elevation of 6,000 feet, as I had hoped to do.

The Earth-light from the Moon when crescent gives a spectrum terminating abruptly at a point very little less refrangible than E, but fades off gradually in the blue, and is traceable to somewhere about 2250 K.

Mangalore,
1874, *January* 27.

The Nebula near η Argûs.

The following is an Extract from a Letter addressed by R. J. Ellery, Esq., to Warren De La Rue, Esq., dated Melbourne, 1874, January 28:—

"We have commenced another drawing of *η Argûs.* It is again quite different from what it was six months ago; the great bay of the lemniscate is nearly filled up with dense nebula leaving a dark sigmoid inlet, which appears *inky* by contrast. Moderately dense nebula has formed from side to side of the opening where *η* is situated, embaying and apparently about to involve that star, so that *η* and the lemniscate are at present in a *lake* (!) completely surrounded by nebula."

On a Determination of Longitude by Chronometers, by the late Lieut. Hastings F. Murphy, R.E. By Robert J. Lecky, Esq.

I have much pleasure in presenting to the Society an original calculation of longitude by chronometer comparisons, made by the late Lieut. Hastings F. Murphy, R.E., astronomer to Colonel Chesney's Euphrates Expedition, who died of fever at Bussorah on the 6th of August 1836, immediately after the successful completion of his arduous duties, and of whom we have an interesting memoir in vol. x of the *Memoirs* and in vol. iv. of the *Monthly Notices*, written by the late Mr. Francis Baily.

Lieut. Murphy held an appointment in the Ordnance Map Office in the Tower; and early in the year 1832 I had a request from him to have correct time ready for him at the Observatory of the Royal Cork Institution, then under my care. He was on leave, and about to visit his father, Rector of Kiltallagh in the county of Kerry, and was bringing with him his own pocket chronometer and two others, which were carefully rated prior to leaving London, as well as while he was at Kiltallagh; and when passing through Cork, both on his outward and return journeys, had good comparisons with me; and on his return to town, sent me the accompanying calculations, which are valuable as a carefully worked-out example of such observations, as well as a memento of a most talented and amiable officer.

He also had a comparison with the late Dr. Dartnell, at Youghal, when passing through that town by coach, but this was only made on one journey, and I also fear that Dr. Dartnell was not sufficiently exact as to the error of his chronometer, or that Lieut. Murphy may not have taken it down correctly as our well ascertained local difference was $2^{m}\ 28^{s}$, while this shows it as $2^{m}\ 48^{s}$. Notwithstanding this error, I leave this calculation attached to that for Cork, and I accompany them with the one-inch scale Ordnance Maps of the places, which give the latitude and longitude with great accuracy. The cross lines in red on these maps show the position of both Observatories; that for

Youghal shows longitude 7° 51′ 10″, or in time $31^m 24^s{\cdot}40$. Lieut. Murphy makes it $31^m 2^s{\cdot}79$; difference $21^s{\cdot}61$, which is a manifest error, and particularly so as the differences for each chronometer correspond very closely. The Cork map gives a longitude of 8° 28′ 12″, or in time $33^m 52^s{\cdot}48$; Lieut. Murphy's being $33^m 52^s{\cdot}74$; the difference being $0^s{\cdot}26$, which is much more likely to have been my error than his, and is, I fancy, a very close result, considering the small number of chronometers employed, and the rougher modes of conveyance in that day compared with those with which we are favoured in the present.

Box Chronometer. "French," No. 3179.

1832	Greenwich to Cork.	Cork to Greenwich.		
	m s	m s		m s
Mean daily rate . .	– 0 0·55	+ 0 0·57		
Elapsed time . . .	0 3·729	0 4·587		
	– 0 2·051	+ 0 2·61		
Civil reckoning.				
Jan. 30. 15½ S. of G. .	– 0 27·6	– 0 46·5	March 18	
	– 0 29·651	+ 0 49·11		
Feb. 3. 9 F. of C. .	33 19·10	33 6·83	March 14	
	33 48·75	33 55·94	Mean	33 52·34
Pocket chronometer, 1894	33 58·50	33 44·73	Do.	33 51·62
Do. 156	33 59·27	33 48·55	Do.	33 53·91
	33 55·51	33 49·74	Do.	33 52·63

Or, mean of 1st chronometer, with mean of 2nd and 3rd . . 33 52·55

Mean of both journeys, giving value to each inversely as elapsed time, viz., G to C, –3·729 days, and C to G, 4·587 do. } 33 52·93

Mean longitude of Cork Observatory, given as nearest to the truth 33 52·74 W.

Longitude of Youghal.

	m s	m s	m s	
1st, No. 3179 =		2 48·88		
2nd, „ 1894 =	2 48·73	2 49·02	2 48·95	Youghal and Cork
3rd, „ 156 =	2 49·30		33 52·74	Cork and Greenwich
			31 3·79	
Dr. Dartnell's chronometer slow, which must be in error			–0 1·00	
			31 2·79	

Observation of Faye's Comet (Comet VI. 1873.) By Dr. C. H. F. Peters.

As there have been no observations of Faye's Comet published, except those made at Marseilles, I have to communicate the following position, the only one that I have obtained:—

	Mean Time Hamilton Coll. h m s	R.A. h m s	Decl. ° ′ ″
1873, Dec. 23	14 10 29	9 18 19·68	−2 15 4·4

The Comet was observed with a ring-micrometer, and the observed place is the mean result of twelve comparisons with a Star whose position is well determined. It was necessary for the air to be very pure to allow the Comet to be seen, and consequently the observation is not so satisfactory as we could have wished. However, the comparison of the observed place with the ephemeris of Dr. Axel Möller gives the following differences:

Calculation − Observation.

$$\text{R.A.} = -0^{s}\cdot37 \qquad \text{Decl.} = +16''\cdot5$$

which confirms the testimony of the observations at Marseilles as to the general accuracy of the ephemeris of Dr. Möller. (*Bulletin International*, March 20, 1874.)

Discovery of Minor Planet (135). By Dr. C. H. F. Peters.

(*Extract from a Letter to the Astronomer Royal.*)

The telegraph will have duly announced the discovery of Asteroid (135) on February 18. Since that date, on account of the weather, I have obtained only one more observation, and have not yet had the opportunity for determining accurately the place of the comparison-star of the first night. The observations are as follow:—

1874	Hamilton Coll. M.T. h m s	R.A. (135) h m s	Decln. (135) ° ′ ″	Number of Comp.
Feb. 18	14 37 49	11 19 42·7	+ 4 25 5	12
„ 24	13 24 45	11 14 36·47	+ 4 53 3·6	10

The planet was compared on February 18 with an anonymous star of the eleventh magnitude, and on February 24 with W.B. (1) xi. 233. The notice given by the telegram of the planet's motion being nearly parallel to the Equator must be modified, as the daily motion is in reality between four and five minutes north. The magnitude was estimated at 11·2.

Litchfield Observatory,
Hamilton College, Clinton, N.Y.,
1874, *February* 26.

Discovery and Elements of Comet I., 1874.

This Comet was discovered by Dr. Winnecke, at Strasburg. in the night of February 20–21, and the observed approximate place, determined from a comparison with a star of the 9th magnitude in the *Bonn Durchmusterung*, +26°. No. 3954, was found to be—

	Mean Time at Strasburg d h m	R.A. h m s	N.P.D. ° ′
1874, February	20 16 0	20 35 0	63 55

From later observations on the same morning, using 27 *Vulpeculæ* as the comparison-star, the place of the Comet was—

	Mean Time at Strasburg. d h m s	R.A. h m s	N.P.D. ° ′ ″	Number of Comp.
1874, February	20 17 16 40·6	20 35 34 10	63 59 15·2	5

Dr. Winnecke observed the Comet also on the two following days, the observed places being—

	Mean Time at Strasburg. d h m s	R.A. h m s	N.P.D. ° ′ ″	Number of Comp.
1874, February	21 17 11 2·7	20 44 29·44	65 25 9·2	6
„ „	22 16 31 6·6	20 53 10·58	66 53 21·7	8

The mean places of the comparison-stars for 1874·0 are—

	R.A. h m s	N.P.D. ° ′ ″	
February 20	20 31 42·06	63 58 31·0	Washington 12-y. Cat.
„ 21	20 42 40·97	65 49 28·2	Bessel
„ 22	20 54 23·43	66 52 47·4	Arg. vol. vi.

The following elements of the Comet have been computed by Dr. Schulhof, of Vienna, from the observations made at Strasburg on February 20, at Pola and Vienna on February 23, and at Pola and Vienna on February 25—

$$T = 1874,\ \text{March}\ 9{\cdot}95342.$$

$$\left.\begin{aligned} \pi &= 300^\circ\ 36'\ 4''{\cdot}2 \\ \Omega &= 31\ \ 31\ \ 18{\cdot}2 \\ i &= 58\ \ 17\ \ 14{\cdot}5 \end{aligned}\right\}\ \text{Mean Equinox, 1874·0.}$$

$$\log. q = 8{\cdot}642852$$

A comparison with the mean observed place gives the following errors:

$$\Delta\lambda \cos\beta = +\ 3''{\cdot}1$$

$$\Delta\beta = -\ 8''{\cdot}2$$

Occultations of Neptune and v^2 Cancri, observed at Mr. Barclay's Observatory, Leyton. By C. G. Talmage, Esq.

Occultation of Neptune on February 20, 1874.

The morning was exceedingly foggy, but by noon the Sun broke out and the fog cleared off, but it came on again in the early afternoon, which, in conjunction with light clouds, prevented the disappearance from being observed. The reappearance at the bright limb took place at $6^h\ 17^m\ 15^s{\cdot}34$ Greenwich M.T. Observed with the 10-inch refractor and power of 180.

Occultation of v^2 Cancri on February 27, 1874.

The star was only occulted by the Moon's limb during eight seconds, but for twenty minutes it ran along the limb, and was frequently occulted for a second or more by lunar elevations.*

	h	m	s
Greenwich M. T. of disappearance	8	33	16·48
„ „ reappearance	8	33	24·48

The clock-error was well-determined on both days; on February 20, from the transit of four stars, and on February 27, from five stars.

* This star was not occulted at the Royal Observatory, Greenwich. It was carefully watched by Mr. Criswick during its progress along the Moon's limb, but on no occasion was it seen to disappear.—[Editor.]

Professorship of Astronomy at the Tung-Wên College, Peking.

The Council have been informed that the office of Professor of Astronomy in this College is now vacant. The duty of taking the students who understand the languages of the West through a course of mathematics in the western text-books would devolve upon the Professor of Astronomy. He would also be expected to teach the applications of mathematics in other departments besides that of Astronomy, and to be willing to do the work of a schoolmaster, and yet be able to take charge of an Observatory, if required to do so. He must be acquainted with German and French.

The salary attached to the Professorship will be 600*l.* per annum for the first five years; 800*l.* for the second five; and 1,000*l.* after the tenth year of service. There will be a residence

provided, with some extra perquisites, including allowances for two servants, medical attendance, &c.

Gentlemen desirous of offering themselves as candidates for the vacant Professorship are requested to communicate with Mr. J. D. Campbell, 17 Talbot Road, Bayswater, London, W.

Discovery of Minor Planet (136).

This planet was discovered by M. Palisa, at Pola, on March 18. The following early observations were made at Pola and Berlin:—

				R.A.	N.P.D.
			h m s	h m s	° ′ ″
Pola	1874	March 18	14 46 39	12 22 2·26	93 19 31·3
"	"	" 19	9 26 50	12 21 22·80	93 11 46·4
Berlin	"	" 21	10 29 57	12 19 37·34	92 50 53·0

Errata.

Vol. xxxiii., page 491, April 21, "Eclipse Reappearance observed by K,' *for* "II. Satellite," *read* "I. Satellite."

Vol. xxxiv., page 62, "Mr. Burnham's Catalogue of Double Stars," No. 110, fourth column, Decl. 1880, *for* "$-16° 32'$," *read* "$-16° 26'$."

No. 112, in notes, *for* "Weisse xii.," *read* "Weisse xii. 1072."

Page 63, No. 120, third column, R.A. 1880, for "$15^h\ 5^m\ 1^s$," *read* "$16^h\ 5^m\ 1^s$."

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

Vol. XXXIV. *April* 10, 1874. No. 6.

Professor Adams, F.R.S., in the Chair.

Edward Hamilton Pringle, Esq., Madras;
John Dyer, Esq., Rokeby Road, New Cross;
Robert Kalley Miller, Esq., Royal Naval College, Greenwich;
Rev. James Pearson, Fleetwood; and
Mortimer Evans, Esq., Glasgow,

were balloted for and duly elected Fellows of the Society.

A proposal has been communicated to the Council for placing, in Westminster Abbey or elsewhere, a Memorial to Jeremiah Horrox, the first observer of a transit of *Venus*. The President intended to mention this at the evening meeting, but inadvertently omitted to do so.

Dry Plate Process for Solar Photography. By Capt. W. de W. Abney, R.E.

Before commencing the preparation of the plates, some fresh eggs (say four for a dozen medium sized plates) are procured, and the whites carefully beaten up (with one drachm of liquid ammonia to each white) by a whisk, a bundle of quill pens, or by shaking in a bottle, into which fragments of glass have been introduced. When the froth has subsided, the clear fluid is procured by filtering through muslin, and is placed in a bottle labelled A. A glass of bitter or mild ale is next obtained, and to half of it (which should be 5 oz.) 10 grains of pyrogallic acid

are added, and the solution, if necessary, is filtered through filter-paper. This is lettered P. The other 5 oz. of beer are placed in another bottle, and labelled B.

Should the fresh eggs not be obtainable, dried albumen may be used; 20 to 25 grains of the latter should be dissolved in an ounce of distilled water, and substituted for them.

Bottled beer may also be substituted for the ordinary bitter ale. Care should be taken that, by a gentle heat, the carbonic acid is all liberated, otherwise carbonate of ammonia will be formed on the addition of alkaline albumen.

(i.) Any ordinary collodion will answer. The bromo-iodized sample, supplied by Thomas of Pall Mall, with 2 grains of pyroxyline added to each ounce, gives very rapid results.

For sun pictures, however, a modification is advisable; and much will depend on the climate in which it has to be employed.

(ii.) For a cold climate, collodion made by the following formula will be found to give good results:—

Thomas's bromized collodion . . .	20 oz.
,, ordinary bromo-iodized . .	20 oz.
Plain collodion, *not* iodized . . .	6 oz.
Pyroxyline	276 grains.
Water	400 minims.

(iii.) For warmer climates the following will be found to answer better:—

Thomas's bromized collodion . . .	20 oz.
,, bromo-iodized	20 oz.
* Alcohol, S. G. ·850	6 to 8 oz.
Pyroxyline	300 grains.
Water	120 minims.

With (i.) the ordinary nitrate of silver bath, 40 grains to the ounce, is used. If greater sensitiveness is required, 10 grains of nitrate of uranium to each fluid ounce of the above are added.

With (ii.) and (iii.) the above bath should be used, together with another made 60 grains to the ounce of water.

A substratum to the collodion is recommended to insure adhesion of the film to the glass plate during development. This is made by mixing the white of one egg with 40 oz. of distilled water, and applying it to the surface of the plate by a piece of swan's down, calico, or flannel, folded over the edge of a strip of glass and used as a brush. The brush is dipped in the fluid, and drawn down the plate in parallel lines till the whole surface has received a coating. Here I may mention that a *clean* plate is necessary; but much polishing with a silk handkerchief or chamois prevents the substratum taking kindly to the glass.

* The hotter the climate, the more alcohol will be required.

Another substratum, which seems to give almost better results than the albumen, may be substituted for the above:—

Sheet gelatine	75 grains.
Distilled water	60 oz.
Ammonia	¼ oz.
Alcohol	1 oz.

The gelatine should be softened in 30 oz. of cold water, and then dissolved by 30 oz. of boiling water. When cold, the remaining ingredients should be added.

If a plate (after the substratum has been thoroughly dried) is coated with collodion (i.), it is sensitized in the ordinary manner in the 40-grain bath, *i.e.*, for about 4 minutes in cold to 2½ in warm weather. If the plate has been coated with (ii.) or (iii.), it is plunged in the 40-grain bath and kept there till all "greasiness" has disappeared. It is then transferred to the 60-grain bath, and kept there for 7 or 8 minutes longer, *i.e.*, until a creamy film is obtained. The plate is next plunged into distilled water, or spring water which has been rendered slightly alkaline by adding a few drops of ammonia to it (if iron be present as an impurity), and to which, *after boiling and filtering*, a few drops of nitric acid have been added to restore neutrality. When the "greasiness" has disappeared from the film, the plate may be washed under the tap for a minute, or in different dishes of water, until all free nitrate of silver is got rid of. (This may be effected rapidly by adding a pinch or two of common salt to the last washing water but one in the dishes.) In a small tumbler are next mixed equal quantities of A and B, stirred up with a glass rod, and floated over the washed film. If all the nitrate have not been washed away, stains may here become manifest. This solution is kept on half a minute, and is then poured off. The plate is once more thoroughly washed, and solution P is floated over for another half minute. The plate is then set up on one corner to dry spontaneously. Before being stored away, the last trace of moisture may be expelled by gently warming over a stove or Bunsen burner. In dry climates this precaution need not be taken. As a rule, the plate requires no "backing" to prevent blurring of the image, but if it appear very transparent a backing may be necessary. Cartridge-paper stained with any red dye (alkaline aurine will answer), and coated with gum and flour stained of the same colour, will give what is required. When damped, the paper will adhere to the back of the plate, and dry in optical contact with it. It can easily be removed by wetting.

The exposure is the same as that necessary for a wet plate prepared with the same collodion, though no damage will be done to the picture if six times that amount be given. With the uranium-bath the dry plate is quicker than a wet plate.

The development need not take place for a month after exposure. The following solutions must be made up:—

No. I.	pyrogallic acid . .	12 grains.
	water	1 oz.
No. II.	liquor ammonia . .	1 part.
	water	4 parts.
No. III.	citric acid . . .	60 grains.
	glacial acetic acid . .	30 minims.
	water	1 oz.
No. IV.	nitrate of silver . .	20 grains.
	water	1 oz.

The plate is washed in spring or rain water of a not less temperature than 60° F. till all the beer has been removed. Sufficient of No. I. is taken to well cover the plate, and first flowed over it. Into the developing cup are then dropped three drops of No. II., and No. I. is poured off the plate on to it. The solution is then flowed over the plate again, and after a few seconds the detail will begin to appear by *reflected* light. As detail appears, another two drops of No. II. may be added, and so on, till *nearly* all the detail is visible. The plate is now washed in water of the same temperature. Here it may be remarked that stronger doses of No. II. may be used to under-exposed pictures. Six drops of No. III. are next dropped into a clean developing cup, and the same quantity of No. I. added as before. This is flowed over the plate to neutralise any trace of ammonia remaining. Into the cup are now dropped two drops of No. IV., the pyro-solution from the plate poured on to it, and once more applied to the film. The image will gradually acquire strength, the remaining detail appearing. The intensity is gained by adding to the same, or fresh (*acid*) pyro-solution more silver (No. IV.) When the image appears of sufficient density, it is fixed with a solution of hyposulphite of soda or cyanide of potassium. In the case where the plate is backed, the film should be wetted first, and then the paper removed.

The alkaline development produces a faint image by the reduction of the organic salt and bromide of silver to the suboxide of silver. The iodide is unattacked by it. The acid silver development utilises the exposed iodide thus: the attraction of the suboxide for fresh silver (deposited by the acid development) is increased by the irritated iodide, and thus density is acquired.

It will be noticed that no restrainer, such as bromide of potassium, is used with the alkaline development. The albumen dissolved by the ammonia plays the part of a retarder, but not of a destroyer. Thus the image is well under control.

An *under*-exposed picture has an image of slate colour; an *over*-exposed picture has one of an olive green; whilst one properly exposed is of a rich chocolate brown. Every plate sufficiently exposed will yield a good negative.

Greenwich,
1874, *7th March.*

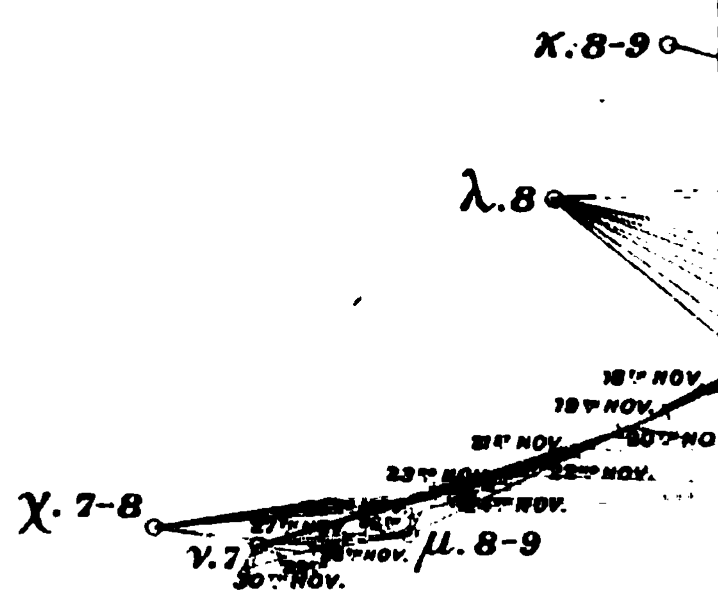
PATH OF

FROM

October 10th. TO Novem

SHEWING S

to which measures sh



On the Determination of the Solar Parallax by Observations of Juno at Opposition. By Lord Lindsay and Mr. David Gill.

Dr. Galle has already proposed (*Astron. Nach.*, vol. lxxx. p. 1), a method of obtaining the solar parallax, by observing, with a wire micrometer simultaneously at a northern and southern observatory, the difference of declination between a minor planet and two stars near the planet, and nearly equally north and south from it.

The method recommends itself, not by a favourable factor of parallax such as is afforded by a transit of *Venus* or a favourable opposition of *Mars*, but by the extreme precision with which a minute point of light can be bisected as compared with that with which a web can be brought into contact with a disk.

We are not aware that any results have been published of the application of the method, though there is no doubt that it is capable of great accuracy.

It has appeared to us that if we can select suitable stars, and observe the parallactic displacement of a minor planet relative to these stars by the Earth's rotation, we have a method of determining the solar parallax, which is free from the difficulties and disappointments attending observations in which co-operation is necessary, and where the most complete arrangements and perfect observation may be upset by unfavourable weather at the opposite station.

The accuracy of the proposed method will depend—

1. On the amount of displacement that can be measured.
2. The accuracy with which the measures can be made.
3. The number of nights on which the measures can be repeated, which will partly depend on the nights on which suitably situated stars of comparison can be found.

The planet *Juno* at the opposition of 1874 appears to be very favourably situated for a trial of this method. Mauritius (the station where we observe the transit of *Venus*) a suitable position, and the Repsold Heliometer, with which the transit is to be observed, a suitable instrument. We have accordingly investigated the conditions of probable attainable accuracy as follows, and trust the results may be acceptable to the Society.

Juno, in opposition on 5th November, 1874, has a horizontal parallax of $8''\cdot7$, and south declination of $3^\circ\cdot50'$.

If we suppose that we may begin to measure when the planet has an altitude of 20°, and continue till the altitude is 40°; and at setting begin to measure at an altitude of 40°, and observe till the planet's altitude is 20°: then for opposition we have—

Altitude.	Parallax in A.R.	Hour Angle.
°	″	h m
20	7·70	4 41
30	7·05	
40	6·14	3 15
	Time available for observation	1 26

In other words, we shall be able to observe a displacement of about 14″, and have 86 minutes both at rising and setting during which we may make observations.

The observations can be made with advantage for a month preceding and a month following opposition.

It will not be possible to observe the planet at so large an hour angle in the early mornings a month before opposition, as the sun rises when the hour angle of the planet is about three hours and a half; nor similarly a month after opposition shall we be able to commence observation so early as is desirable, and moreover the horizontal parallax will be a little less. Notwithstanding this, for fifty nights an average parallactic displacement in A.R. of about 12″ can be obtained.

In the list of stars which we have prepared for comparison with *Juno*, distances will be found considerably exceeding those usually measured with the heliometer.

So far as we know, *Bessel* seldom, if ever, measured distances equal to 3000″, and at Oxford the greatest distances measured with the heliometer have been under that amount.

In the Königsberg heliometer (*Bessel's*), the divided lenses being mounted in plain slides, distortion of the image resulted when large angles had to be measured; but in the construction of the new Repsold heliometer, as also in the Oxford heliometer by the same makers, the lenses move in slides of circular curvature in the direction of their length, the centre of curvature being the focal point of the object-glass.

The effect of this arrangement is, that whatever the separation of the segments of the object-glass, the images are equally free from distortion, and so far as the optical qualities of the instrument are concerned, a large angle can be measured with equal accuracy with a small one.

In the use of the Oxford heliometer, as in the Königsberg instrument, one segment of the object-glass remains fixed, and the other is used for measurement on opposite sides of the fixed segment.

In this way the extent of angle which the instrument is capable of measuring is limited by the amount which one segment can be moved on either side of zero.

In the more modern form of the Repsold heliometer* both

* Astronomy is indebted to Drs. Struve, Auwers, and Winnecke for the arrangement of this instrument. Similar heliometers are to be employed by Russia for observation of the transit of *Venus*.

halves of the divided object-glass are moved in equal and opposite directions by a single movement, and thus the extent of angle which can be measured is doubled, and the instrument can measure completely an angle of 2°.

The symmetry thus given to the observation also facilitates the reduction of the observations and adds to their accuracy, but to this we need not further allude at present. It will be sufficient to show with what precision such angles can be measured, and what precautions are necessary to attain the highest possible accuracy.

In illustration, we give the following observations recently made by Mr. Gill, at Dunecht Observatory, on a zone of stars in *Perseus* * to determine the value of the scale of the instrument, the distances and magnitudes of the stars being very similar to those which occur in the list of stars to be measured with *Juno.*

The *first* column contains the date of observation.

The *second*, the hour angle of observation.

The *third*, the distance converted into arc on an assumed approximate value of the heliometer scale.

The *fourth*, the sum of the corrections for aberration and refraction (the effect of the barometer and thermometer on the refraction has been included).

The *fifth*, the resulting distance.

The *sixth*, the mean of each successive pair of observations which constitute a *complete* measure. (See note * on next page.)

The *seventh*, the difference of the *complete* observation from the mean of the *complete* observations of the same evening.

The *eighth*, the difference from the mean of all the observations of the same object.

The *ninth*, the temperature at the time of observation.

* The stars are as follow:—

	Mag.	A.R. h	m	s	Decl. °	′
a	6·8	3	8	55	+40	1·2
b	7·5	3	5	53	39	37·7
c	5·2	3	3	7	39	7·9
d	8·1	2	59	26	38	34·9
e	5 var.	2	57	4	38	20·8
f	6·8	2	53	50	38	23·8
g	6·0	2	50	2	38	5·7
h	6·0	2	45	44	37	49·3
i	4·8	2	42	36	37	47·2
k	7·0	2	39	35	37	16·2

The successive distances and position-angles of these stars having been measured, the distances are projected on the great circle joining the extreme stars, and the result so obtained is compared with the distance obtained by Meridian observation of the extreme stars. The same stars have been observed by the German heliometer observers of the transit of *Venus.* The same

c—e

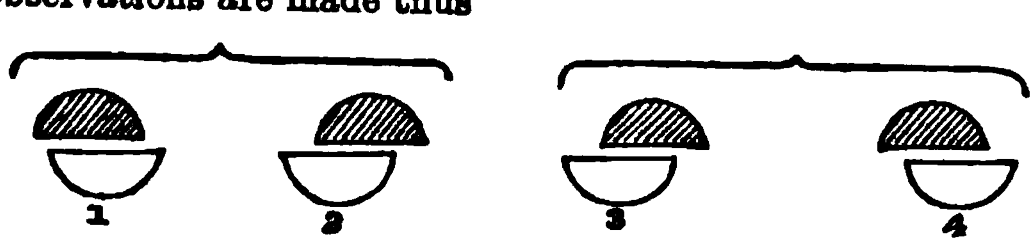
I. Date.	II. Hour Angle.	III. Observed Distance.	IV. Correction for Refraction and Aberration.	V. Resulting Distance.	VI.	VII.	VIII.	IX.
	h m	″	″	″	″	″	″	° ′
Feb. 3	1 10·3	5094·811	+1·663	5096·474	5096·276	...	0·358	38 3
	1 19·5	5094·386	1·691	5096·077				
Feb. 10	3 13·9	5094·669	2·261	5096·930	5096·774	0·022	0·140	29 0
	3 23·0	5094·286	2·331	5096·617				
	3 33·3	5094·095	2·423	5096·518	5096·818	0·022	0·184	
	3 39·8	5094·582	2·536	5097·118				
Feb. 16	2 36·9	5094·618	1·897	5096·515	5096·564	0·141	0·070	40 0
	2 43·5	5094·677	1·935	5096·612				
	2 57·5	5094·360	2·017	5096·377	5096·282	0·141	0·352	
	3 4·0	5094·131	2·055	5096·186				
Feb. 19	2 22·5	5095·202	1·885	5097·087	5096·913	0·050	0·279	40 5
	2 29·0	5094·826	1·913	5096·739				
	3 26·5	5094·710	2·282	5096·992	5096·814	0·050	0·180	38 0
	3 32·5	5094·301	2·335	5096·636				

e—g

I. Date.	II. Hour Angle.	III. Observed Distance.	IV. Correction for Refraction and Aberration.	V. Resulting Distance.	VI.	VII.	VIII.	IX.
Feb. 10	1 33·0	5061·521	−1·628	5063·149$^{5}_{4}$	5063·063	0·099	0·066	28 0
	1 40·7	5061·328	1·648	5062·976				
	1 53·5	5061·925	1·689	5063·614	5063·261	0·099	0·264	
	2 1·2	5061·210	1·698	5062·908				
Feb. 13	2 28·1	5061·223	1·678	5062·901	5063·022	0·136	0·025	40 2
	2 36·9	5061·421	1·721	5063·142				
	2 46·1	5061·151	1·748	5062·899	5062·750	0·136	0·247	
	2 53·1	5060·824	1·777	5062·601				

places for the extreme stars will be assumed in the reductions, and the values of the scales of the heliometers so obtained will thus be strictly comparable, and a small absolute error in their values would have no effect on the transit of *Venus* observations. The star *a* has been rejected, as it is a difficult double star, and *b* to *k* are the extreme stars.

* The observations are made thus



with the halves of the object-glass as sketched. Observations 1 and 2 give one measure, and 3, 4 another; but as the zero of the microscope is continually changing from flexure, it is necessary to make the observations in this order to eliminate the effect of this change.

I. Date.	II. Hour Angle.	III. Observed Distance.	IV. Corr. for Refraction and Aberration.	V. Resulting Distance.	VI.	VII.	VIII.	IX.
	h m	″	″	″	′	″	″	° ′
Feb. 18	3 48·2	5060·811	2·143	5062·954	5062·890	...	0·107	35 0
	3 55·0	5060·636	2·190	5062·826				
			h—i					
Jan. 9	22 37·5	2226·448	+0·614	2227·062	2227·218	...	0·211	38 0
	22 56·1	2226·757	0·616	2227·373				
Jan. 24	0 36·2	2225·939	0·665	2226·604	2226·878	0·024	0·129	31 0
	0 45·7	2226·479	0·673	2227·152				
	1 3·9	2226·366	0·693	2227·059	2226·926	0·024	0·081	
	1 16·2	2226·095	0·697	2226·792				
			g—h					
Jan. 9	23 26·5	3213·297	+0·908	3214·205	3214·400	0·168	0·044	39 0
	23 38·6	3213·685	0·909	3214·594				
	23 58·1	3213·438	0·914	3214·352	3214·063	0·168	0·293	
	0 9·8	3212·856	0·918	3213·774				
Jan. 24	1 35·0	3213·029	1·055	3214·084	3214·147	0·227	0·209	30 7
	1 44·3	3213·139	1·070	3214·209				
	1 55·5	3213·477	1·093	3214·570	3214·600	0·227	0·244	
	2 5·8	3213·508	1·121	3214·629				
Feb. 16	3 55·0	3213·379	1·463	3214·842	3214·568	...	0·212	39 8
	4 5·9	3212·766	1·527	3214·293				
			h—k					
Feb. 10	2 32·8	4841·915	+1·868	4843·783	4843·766	0·089	0·428	28 0
	2 42·8	4841·828	1·921	4843·749				
	2 58·3	4841·380	2·013	4843·393	4843·589	0·089	0·605	
	3 6·9	4841·715	2·070	4843·785				
Feb. 13	4 21·7	4841·424	2·575	4843·999	4844·195	...	0·001	41 3
	4 28·1	4841·735	2·656	4844·391				
Feb. 16	4 32·5	4842·309	2·719	4845·028	4844·546	0·165	0·352	39 0
	4 41·3	4841·202	2·861	4844·063				
	4 52·3	4842·664	3·065	4845·729	4844·876	0·165	0·682	
	5 2·2	4840·742	3·280	4844·022				
			b—c					
Jan. 4	0 47·9	2683·868	+0·867	2684·735	2684·937	0·013	0·149	31 0
	1 0·9	2684·260	0·878	2685·138				

I. Date.	II. Hour Angle.	III. Observed Distance.	IV. Corr. for Refraction and Aberration.	V. Resulting Distance.	VI.	VII.	VIII.	IX.
	h m	"	"	"	"	"	"	° ′
	1 20·6	2684·131	0·899	2685·030	2684·962	0·013	0·124	
	1 37·3	2683·964	0·930	2684·894				
Feb. 3	1 36·8	2684·528	0·923	2685·451	2685·440	0·097	0·354	38 0
	1 44·5	2684·489	0·940	2685·429				
	1 54·0	2683·660	0·958	2684·618	2685·247	0·097	0·162	
	2 0·3	2684·903	0·972	2685·875				
Feb. 28	2 53·4	2683·940	1·082	2685·022	2684·942	0·022	0·144	41 0
	3 0·9	2683·753	1·108	2684·861				
	3 11·3	2684·038	1·142	2685·180	2684·985	0·022	0·101	
	3 19·0	2683·616	1·174	2684·790				

c—d

Feb. 3	2 28·0	3222·782	+1·246	3224·028	3223·494	0·054		37 6
	2 34·2	3221·693	1·266	3222·959				
	2 42·9	3222·710	1·296	3224·006	3223·602	0·054		
	2 52·8	3221·871	1·327	3223·198				

e—f

Jan. 9	1 28·5	2291·022	+0·677	2291·699	2291·465	0·202	0·303	38 0
	1 36·7	2290·551	0·680	2291·231				
	1 49·4	2290·865	0·689	2291·554	2291·869	0·202	0·101	
	2 0·4	2291·485	0·698	2292·183				
Jan. 25	1 56·1	2291·061	0·694	2291·755	2291·966	...	0·198	44 5
	2 12·4	2291·470	0·707	2292·177				
Feb. 4	0 10·1	2291·053	0·661	2291·714	2291·727	0·043	0·041	43 5
	0 19·4	2291·076	0·664	2291·740				
	0 49·6	2291·210	0·672	2291·882	2291·812	0·043	0·044	
	0 57·6	2291·068	0·674	2291·742				

f—g

Jan. 9	0 39·7	2863·227	+0·657	2863·884	2863·537	0·182	0·186	38 5
	0 48·2	2862·530	0·659	2863·189				
	1 5·3	2862·167	0·660	2862·827	2863·173	0·182	0·178	
	1 15·2	2862·849	0·669	2863·518				
Jan. 25	1 9·9	2862·471	0·665	2863·136	2863·464	0·117	0·113	44 5
	1 17·9	2863·122	0·670	2863·792				
	1 36·9	2862·484	0·682	2863·166	2863·231	0·117	0·120	
	1 45·3	2862·610	0·685	2863·295				

d—e

I. Date.	II. Hour Angle.	III. Observed Distance.	IV. Corr. for Refraction and Aberration.	V. Resulting Distance.	VI.	VII.	VIII.	IX.
	h m	″	″	″	″	″	″	° ′
Feb. 3	3 14·8	1885·239	+0·820	1886·059	1886·439	...	...	37 0
	3 22·1	1885·980	0·838	1886·818				

i—k

Jan. 4	23 18·5	2905·021	+0·875	2905·896	2906·166	0·075	0·051	30 0
	23 33·1	2905·552	0·884	2906·436				
	23 50·9	2905·261	0·893	2906·154	2906·315	0·075	0·200	
	0 4·7	2905·572	0·904	2906·476				
Jan. 17	23 9·4	2905·166	0·842	2906·008	2905·953	0·093	0·162	32 0
	23 24·9	2905·045	0·852	2905·897				
	23 41·5	2904·697	0·860	2905·557	2905·769	0·093	0·346	
	23 54·0	2905·117	0·863	2905·980				
March 4	2 48·6	2905·209	1·195	2906·404	2906·361	0·169	0·246	47 3
	2 58·8	2905·081	1·237	2906·318				
	3 11·0	2904·728	1·295	2906·023	2906·124	0·169	0·009	
	3 18·6	2904·887	1·337	2906·224				

If we examine the seventh column, which contains the difference of each complete observation from the mean of the complete observations of the same evening, and compare these differences with those in the eighth column, where we have the difference of each complete observation from the mean of the whole observations made on the same stars, we find, as we should expect, that the observations of the same evening agree with each other much more closely than those of different evenings.

Dividing the stars into three groups, according to their distances, we find by the method of least squares the probable error from the residuals of column VII. between observations of the same object made within a short interval of each other, representing *very nearly* the probable error of *pointage* (which includes the error in making coincidence of the images of the stars and error in reading the scales *) and from the residuals in column VIII. the probable error of measure, including errors produced by temperature, &c., on different evenings. The results are:

* The error of runs is practically eliminated in the method of observation by observing the division which precedes as well as that which follows the zero of the microscope.

	Mean Distance.	Probable Error by Column VII.	Probable Error by Column VIII.
	″	″	″
Group I.	2401	0·069	0·123
Group II.	3052	0·088	0·137
Group III.	5001	0·081	0·220

We find from these "probable errors" a confirmation of what we previously stated, that the error of *pointage* is hardly affected by the distance of the objects, whilst the other errors of observation from night to night vary nearly proportionally with the distance between the stars.

It becomes important to consider very fully what are these sources of error which in the observation of stars of about 5000″ distance increase the probable error of observation nearly threefold.

The errors to which an observation with the heliometer is subject, beyond those of *pointage*, appear to be due to—1st, Temperature. 2nd. Maladjustment of the focal point. 3rd. Errors of division of the scales. 4th. Errors of the micrometer screw, used to subdivide the scale intervals. 5th. Error of the absolute value of the scale at the standard temperature.

Of these, we shall at present only deal with 1 and 2; 3 and 4 will also to a small extent affect the results, as two observations of the same evening are usually made with the position circle reversed, and the flexure of the tube as well as a small "loss of time" in the slow motion of the segments of the object-glass combine to change the zero of the microscope somewhat, and so a different part of the micrometer screw is used in the measures, and even sometimes another division. The investigation of the errors of the micrometer screw are not yet complete, but the effect of their application can only be to diminish very slightly the probable error. 5. Can only affect us where *absolute* results are required, and these do not enter into the question before us.

To obtain some first approximate idea of the temperature correction, we took the intervals of the stars observed at the highest temperature and compared them with the intervals of lowest temperature, as follows—dividing the observations into two series, one of double, the other of single intervals:

Distance.	Temperature.			Distance.	Temperature.			Difference.
″	°	′		″	°	′		
5096·796	29	0	F.	5096·864	39	0	F.	+·00133
5063·162	28	0	,,	5062·886	40	2	,,	−·00447
4843·678	28	0	,,	4844·195	41	3	,,	+·00803
2226·902	31	0	,,	2227·218	38	0	,,	+·02024
3214·397	30	7	,,	3214·230	39	0	,,	−·00614
2684·950	31	0	,,	2684·964	41	0	,,	+·00052
2291·667	38	0	,,	2291·770	43	5	,,	+·00814
2863·355	38	5	,,	2863·348	44	5	,,	−·00041
2906·241	30	0	,,	2906·293	47	0	,,	+·00004

From these we obtain the following results, having regard to the distances and temperatures:

1st Series, effect of + 1° F. on 1000″ distance = +0″·00174
2nd Series, " " " " = +0″·00189

It is evident, however, from inspection of the residuals that this close coincidence of results is greatly indebted to chance, and the only conclusion we can draw from it is that the temperature correction of the instrument is excessively small, and that a very much greater range of temperature than that afforded by the recent mild winter will be necessary to obtain this coefficient with the desirable accuracy.

If we consider in what way temperature affects the value of the heliometer scale, we shall see why this coefficient is so small. The value of the scale depends on the proportion which the length of the scale bears to the focal length of the telescope.

This scale being of silver the effect of temperature on it will be represented by the usual coefficient of expansion of that metal. The effect of temperature on the focal length of the object-glass has been measured as follows:—In the common focus of the object-glass and eye-piece are cross webs. To the frame carrying these webs is attached a scale, divided into $\frac{1}{10}$ of millimètres, and these divisions can be read by estimation to $\frac{1}{100}$ of a millimètre relative to a line engraved on a block attached to the brass tube of the telescope, so that the readings of the focal point by this scale at different temperatures give measures of the focal point relative to the brass tube of the instrument.

The following are the results of these measures:

Temperature.	No. of Obs.	Millim.
32°–36° F.	23	7·940
38–42 "	41	7·937
42–48 "	21	7·934

The lengthening of the focus of the object by heat, is thus found to be practically equal to the expansion of the brass tube of the instrument.

This law appears to hold good with many instruments. Struve found the same with the Prime Vertical Transit at Pulkowa, and the German heliometer observers have found similar results with their instruments. The temperature coefficient of the instrument is therefore equal to—

(" coefficient of expansion of silver ")—(coefficient of expansion of brass).

If we take the coefficient of expansion for 1° F. for

Silver = ·00001060
Brass = ·00001044

the temperature coefficient of the instrument will be + ·00000016, or for our former unit of 1000″ = 0·″00016 for 1° F.

This quantity is so minute as to be almost insensible in the largest angles and greatest differences of temperature likely to be measured, amounting only to 0·″056 for a distance of 5000″ and a difference of 70° from mean temperature. As the silver of the scale is probably alloyed with copper, whose expansion is lower than that of brass, it is probable that the expansion of the scale is somewhat less than we have assumed and the equality of expansion of the scale and tube even still more perfect.

It is quite otherwise, however, when there is a *difference* of temperature between the object-glass and scale: then the excess of the temperature of the scale over that of the object-glass will increase its value in direct proportion to the excess of temperature and coefficient of the scale's expansion. Thus, if the temperature of the scale exceeds that of the object-glass by 5° F. the increase of the value of the scale (that is, the diminution of its reading) is 0″·261 on a distance of 5000.″

We shall now examine the effects of the maladjustment of the focus.

Suppose a measure correctly made at the proper focus, let s be the distance observed, f the focal length of the heliometer. If we now suppose the observer's eye to have no accommodation and remove the eye-piece from the object-glass by a very small amount $= \frac{f}{n}$, we shall disturb the coincidence of the images, and to restore that coincidence shall have to separate the divided segments still further by an amount $= \frac{s}{n}$, so that for an observation where the focal point is too great by an amount $= \frac{f}{n}$, the correction will be $= -\frac{s}{n}$ . . . . (1).

As in observations of stars there is generally little difference between the temperature of the object-glass and that of the tube, it has been usual to make all the measures using the same readings of the focal scale, so that the position of the focal point with respect to the cross webs depends on the difference in temperature of the tube and object-glass.

If we denote—

t^g = temperature of the object-glass,
t^s = " " " scale,
t^t = " " " tube,
t = normal temperature at which the focal point is adjusted and scale value determined,
x = coefficient of expansion of scale,
y = " " " " tube,
z = lengthening of focal length of object-glass by 1° F.,
s = distance measured,

we shall have—
Correction for temperature of scale $= + x\,(t^s - t)\,s$.

If we put $x = y + \Delta\, y$,
The correction becomes$= -y\,(t^s - t)\,s + \Delta\, y\,(t^s - t)\,s$ . . . (2).
The correction for temperature of the object-glass is $= -z(t^s - t)\,s$.

But since $z = y$ the correction may be written $=$
$- y(t^s - t)\,s$ . . . . (3).

Since the focal point is adjusted for temperature t, we must employ a correction for the displacement.

The increase of the length of the tube is $f\,(t^t - t)\,y$.

And that of the focus of object-glass

$$= f\,(t^s - t)\,z = f(t^s - t)\,y.$$

The difference is the distance which the eye-piece will be removed beyond the focal point $\left(\frac{f}{n}\right) = f\,(t^t - t^g)\,y$.

But by (1) the correction for $\frac{f}{n} = -\frac{s}{n}$.

Hence the correction for displacement of focus becomes
$- y\,s\,(t^t - t^g)$ . . . . (4).

The sum of these corrections, 2, 3, and 4, is the correction to be applied to an observation.

$$2 = -syt^g + syt$$
$$3 = +syt^s - syt + \Delta y.(t^s - t)s$$
$$4 = -syt^t + syt^g$$

The sum of which is—

$$sy(t^s - t^t) + \Delta\, y(t^s - t)\,s$$

Thus we see it is only necessary to observe the temperatures of the scale and tube in order to eliminate all the effects of temperature on the measures, *if we assume that the eye has no accommodation.*

This could almost be done for very minute quantities if we could illuminate the field, and focus the eye on the wires immediately before measuring; but in the measurement of 8th and 9th magnitude stars it is necessary to employ a perfectly dark field. Some small systematic errors therefore creep in on this account, and it will be necessary so to arrange the stars of comparison and the order of observation that this may be eliminated as far as possible.

In constructing the following list we have endeavoured to select stars of nearly equal declination, and equally opposite A.R. from the planet, and sufficiently bright for observation. If this condition could be fulfilled in every case, the observations would be subject to no systematic error. It is intended, both at rising and setting, to make first one *complete* comparison with the star preceding the planet, next one with the star following, again another with the star following, and finally one with the star

preceding. In this way the effect of any constantly increasing instrumental change will be completely eliminated. The effect of a minute change in the value of the scale between rising and setting will be to give too large a parallax by one of the stars, and too small a parallax by the other, the mean would be free from systematic error. It has, of course, been impossible to select stars strictly fulfilling this condition; generally, at least, the stars are on nearly exactly opposite sides of the planet, though not exactly in the same parallel of A.R., in which case the parallactic displacement will depend to some extent on the position-angles. If the stars are strictly opposite and equally distant from the planet, a change of zero of the position circle is completely eliminated in the mean result. A glance at the accompanying map will show how we have endeavoured to overcome systematic errors by the arrangement of the stars of comparison. On several occasions where the star is very close to the planet, the systematic error must be so small that it has been thought better to measure only one very small distance rather than two very great and nearly opposite distances.

In order to eliminate the effect of an unknown error of the equatoreal adjustment on the position-angles, two meridian marks placed in the focus of lenses of long focus will be employed, one for "telescope following," and one for "telescope preceding."

The declination axis being levelled by a spirit level, a single observation of each mark will give the azimuth and altitude of the pole of the instrument (the azimuth and inclination of the collimator and the horizontal flexure of the heliometer being known).

The error of zero of the position circle will be obtained by separating the lenses, and bringing the image of the collimating mark to bisect successively the cross webs, when the heliometer is turned on the declination axis: this observation should be made with the tube on both sides of the polar axis.

By these precautions it is believed that when the distance is small, the parallax determined by the position-angles will be equally reliable with that obtained from the distances; particularly as in small stars the elongation of the images produced by division of the object-glass is hardly perceptible.

There are still two sources of systematic error which must be carefully eliminated: the *first* arises from an error in the assumed motion of the planet; the *second* from the errors of division of the scale and the errors of the micrometer screw, since by the motion of the planet different divisions will be employed at rising and setting.

With regard to the *first* of these, we are assured by Mr. Hind that it will be possible, by the existing observations of the planet, so to correct the present tables that not the smallest error is to be feared from this cause.

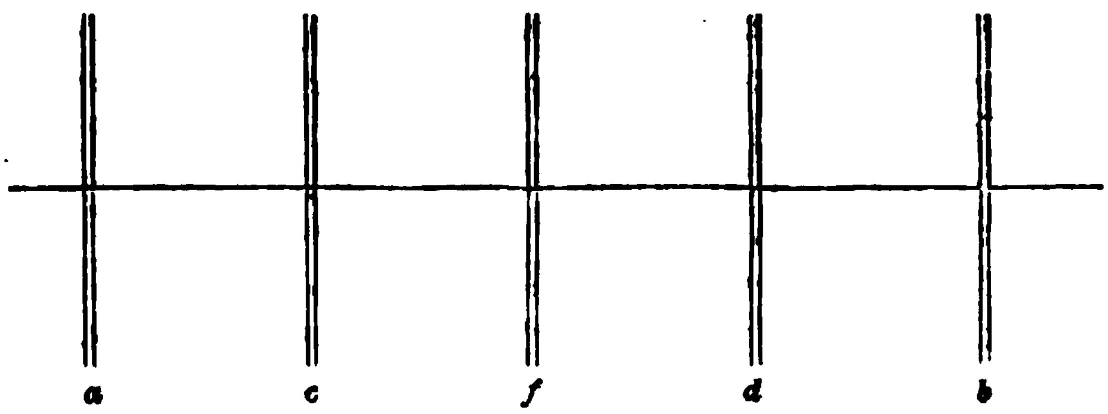
With regard to the *second*, it is not necessary for our purpose to ascertain the *absolute* division error of every interval. The division errors of our heliometer scale, as we shall presently

show, have been investigated for all divisions which occur in the measurement of angles up to 40′, with every desirable accuracy, so that it will only be necessary to compare the divisions passed over during the night's observation with several divisions whose interval is known.

The errors of the screw are now being investigated in a manner similar to that so successfully employed by the late Prof. Kaiser at Leiden, and though the results are not yet completed, there is no doubt it is simply a question of time and care to ascertain these errors with any accuracy that is required.

In the determination of errors to which we have alluded, a portion of each scale from division 50 to division 98 has been selected.

In the moveable wire frame of the micrometer Mr. Simms inserted at our request double webs as follow:



The space from a to b being slightly less than twelve divisions, and that from c to d slightly less than six divisions.

The first process was to subdivide each scale into four parts of twelve divisions each: the interval of a twelve-division interval exceeding the interval $a\,b$ by an amount $= n^1\ n^2\ n^3\ n^4$ successively. Then the true length of a 4th part of scale A will be $a\,b + \dfrac{n^1 + n^2 + n^3 + n^4}{4}$: the difference of $a\,(b + n^1)$ from a 4th part of scale A is the excess of the length of the interval 50–62 over $\dfrac{\text{Scale A}}{4}$.

In Table I., 1st, 2nd, 5th series, column 1, will be found these excesses of the intervals found by different series of observations in terms of the micrometer screw. The same point of the screw being used throughout, no error will result from this cause: the weights depend partly on the number of observations, and partly on the method of making the observations; but as the full details will afterwards be published, we need not further allude to them at present.

The results of the 5th and 7th series are derived from observations made with the wires of interval $c\,d$, that is, by dividing the scale into 8 parts: the weight of this series has been made one half the other, in proportion to the number of observations. We believe this weight should be still less.

Column 2 gives the difference from mean in *seconds of arc*,

and the accuracy of the observations may be judged of from the amount of these residuals.

Table II. similarly exhibits the results of subdivision into intervals of six divisions; and Table III. that of subdivision into intervals of three divisions: in the case of the latter, instead of the residuals, we give the probable error of the bisection of the interval of six divisions in *seconds of arc*.

From these we obtain the successive division errors for each third division reckoned from 50 towards 98, in seconds of arc,*

Scale A $0{\cdot}000 + 0''{\cdot}162 + 0''{\cdot}242 + 0''{\cdot}017 + 0''{\cdot}205 + 0''{\cdot}242 + 0''{\cdot}266 + 0''{\cdot}315 + 0''{\cdot}013 + 0''{\cdot}113 + 0''{\cdot}168 + 0''{\cdot}117 + 0''{\cdot}108 - 0''{\cdot}048 - 0''{\cdot}027 - 0''{\cdot}038 : 0{\cdot}000$.

Scale B $0{\cdot}000 - 0''{\cdot}060 + 0''{\cdot}113 + 0''{\cdot}138 + 0''{\cdot}010 + 0''{\cdot}088 + 0''{\cdot}161 + 0''{\cdot}027 + 0''{\cdot}152 + 0''{\cdot}074 + 0''{\cdot}212 + 0''{\cdot}217 - 0''{\cdot}662 - 0''{\cdot}062 - 0''{\cdot}021 + 0''{\cdot}011 \pm 0{\cdot}000$.

If we look at these division errors, we see how very beautifully the scales are divided, and yet how much in a delicate enquiry like the present our systematic errors would be increased by disregarding them.†

We cannot pretend to predict what the probable error of a series of observations on the parallax of *Juno* will be, but if on 25 nights we can determine, as we believe we can, the place of the planet to $0''{\cdot}1$, the resultant error in the solar parallax will be about

$$\frac{\frac{8{\cdot}9}{12} \times 0''{\cdot}1}{\sqrt{25}} = 0''{\cdot}017$$

and we believe that this method is capable, with the precautions we have given, to give a result exceeding in accuracy that by any other method, if a sufficient number of observations on a number of different planets could be obtained.

* 1 Division is equal to 2 revolutions of the micrometer screw, and 1 revolution of the micrometer screw is approximately $= 25''{\cdot}74$.

† We have been led to give more fully than we otherwise should have done in the present paper some account of the determination of these division errors, from a remark of the Astronomer Royal, contained in his Report to the Board of Visitors, read June 3, 1873. Sir George Airy says, "My present impression is one of doubt on the certainty of equality of the parts of the scale employed." If we take even the uncorrected scale the probable error of a division hardly exceeds that of an error of four seconds of time in an observation of contact in the transit of *Venus*, and the fact that eight divisions are used in each measurement diminishes the probable error to a much smaller amount. We have not had time to fully compute the probable error of determination of the division error of each scale, but Tables I., II., and III. show it cannot exceed 2 or 3 hundredth parts of a second of arc.

Excesses of Division Intervals.

Scale A. Table I.

	Weight	50–62 1	50–62 2	62–74 1	62–74 2	74–86 1	74–86 2	86–98 1	86–98 2
1st series	12	+0107	0″·069	−0080	0″·013	+0032	0″·013	−0059	0″·044
2nd series	12	+0085	0·013	−0066	0·023	+0021	0·041	−0040	0.005
5th series	2	+0055	0·064	−0045	0·077	+0042	0·013	−0052	0·026
6th series	32	+0080	0·000	−0079	0·010	+0046	0·023	−0047	0·013
7th series	8	+0035	0·116	−0068	0·018	+0031	0·015	+0002	0·113
		+0080		−0075		+0037		−0042	

Scale B. Table I.

	Weight	50–62 1	50–62 2	62–74 1	62–74 2	74–86 1	74–86 2	86–98 1	86–98 2
1st series	12	+0045	0″·018	+0034	0″·033	−0037	0″·005	−0042	0″·046
2nd series	12	+0021	0·044	+0020	0·003	−0025	0·026	−0016	0·021
5th series	2	+0030	0·021	+0002	0·049	−0032	0·008	−0000	0·062
6th series	32	+0053	0·031	+0020	0·003	−0042	0·018	−0028	0·010
7th series	8	+0009	0·075	+0009	0·031	−0016	0·049	−0002	0·057
		+0038		+0021		−0035		−0024	

Scale A. TABLE II.

	Weight	50–56		56–62		62–68		68–74		74–80		80–86		86–92		92–98	
		1	2	1	2	1	2	1	2	1	2	1	2	1	2	1	2
3rd series	1	+0050	0″·010	−0050	0″·010	+0083	0″·057	−0083	0″·057	+0042	0″·000	−0042	0″·000	−0032	0″·003	+0032	0″·003
4th series	1	+0069	0·039	−0069	0·039	+0074	0·033	−0074	0·033	+0035	0·018	−0035	0·018	−0018	0·033	+0018	0·033
5th series	1	+0053	0·003	−0053	0·003	+0092	0·080	−0092	0·080	+0036	0·015	−0036	0·015	−0002	0·075	+0002	0·075
7th series	2	+0049	0·013	−0049	0·013	+0028	0·085	−0028	0·085	+0048	0·015	−0048	0·015	−0053	0·057	+0053	0·057
Error of interval in terms of series.		+0054	...	−0054	...	+0061	...	−0061	...	+0042	...	−0042	...	−0031	...	+0031	...

Scale B. TABLE II.

	Weight	50–56		56–62		62–68		68–74		74–80		80–86		86–92		92–98	
		1	2	1	2	1	2	1	2	1	2	1	2	1	2	1	2
3rd series	1	+0050	0″·064	−0050	0″·064	+0022	0″·021	−0022	0″·021	−0005	0″·118	+0005	0″·118	+0036	0″·021	−0036	0″·021
4th series	1	+0001	0·061	−0001	0·061	+0006	0·021	−0006	0·021	+0028	0·033	−0028	0·033	+0033	0·013	−0033	0·013
5th series	1	+0029	0·010	−0029	0·010	+0025	0·028	−0025	0·028	+0061	0·051	−0061	0·051	+0041	0·033	−0041	0·033
7th series	2	+0022	0·007	−0022	0·007	+0009	0·013	−0009	0·013	+0055	0·036	−0055	0·036	+0016	0·031	−0016	0·031
Error of interval in terms of series.		+0025	...	−0025	...	+0014	...	−0014	...	+0041	...	−0041	...	+0028	...	−0028	...

Scale A. TABLE III.

	50–53	53–56	56–59	59–62	62–65	65–68	68–71	71–74	74–77	77–80	80–83	83–86	86–89	89–92	92–95	95–98
1st series . .	+0032	−0032	−0078	+0078	0000	0000	+0052	−0052	+0010	−0010	+0001	−0001	−0055	+0055	−0025	+0025
2nd series. .	+0094	−0094	−0042	+0042	−0032	+0032	+0047	−0047	+0026	−0026	+0001	−0001	−0035	+0035	−0015	+0015
Probable error	0″·056	0″·056	0″·033	0″·033	0″·029	0″·029	0″·005	0″·005	0″·015	0″·015	0″·000	0″·000	0″·018	0′·018	0″·009	0″·009

Scale B. TABLE III.

	50–53	53–56	56–59	59–62	62–65	65–68	68–71	71–74	74–77	77–80	80–83	83–86	86–89	89–92	92–95	95–98
1st series . .	−0015	+0015	+0024	−0024	−0017	+0017	−0048	+0048	−0050	+0050	+0015	−0015	−0002	+0002	+0030	−0030
2nd series. .	−0032	+0032	+0020	−0020	−0005	+0005	−0042	+0042	−0051	+0051	+0022	−0022	−0026	+0026	+0023	−0023
Probable error	0″·015	0″·015	0″·004	0″·004	0″·011	0″·011	0″·005	0″·005	0″·001	0″·001	0″·006	0″·006	0″·022	0″·022	0″·006	0″·006

List of Stars to be observed with Juno.

Name of Star.	Mag.	Mean R.A. 1874, Jan. 1. h m s	Mean Decl. 1874, Jan. 1. ° ′ ″	Name of Star.	Mag.	Mean R.A. 1874, Jan. 1. h m s	Mean Decl. 1874, Jan. 1. ° ′ ″
a	7–8	3 20 22	−0 24 8	*x*	8–9	3 5 43	−2 55 1
b	8–9	3 18 25	+1 31 9	*y*	6–8	3 5 15	−0 58 0
c	8–9	3 18 14	+0 54 6	*z*	6–7	3 5 1	−4 17 1
d	7	3 17 7	+0 28 0	α	7–8	3 3 55	−2 10 3
e	9	3 14 45	−0 58 7	β	9	3 3 7	−3 42 8
f	8–9	3 13 39	−2 2 9	γ	8	3 2 42	−4 7 6
g	7–8	3 13 32	+1 1 6	δ	9	3 2 26	−4 10 0
h	7–8	3 13 16	+0 44 5	ε	9	3 2 28	−4 4 4
i	7–8	3 12 38	−3 17 7	ζ	8–9	3 1 6	−5 52 2
k	5–6	3 11 54	−1 23 2	η	6–8	3 0 50	−2 17 1
m	8–9	3 9 3	−2 38 5	θ	8	2 57 55	−4 49 1
n	9	3 9 7	−0 56 6	ι	7	2 57 53	−5 44 0
o	7–8	3 8 13	−2 48 1	κ	8–9	2 53 48	−4 10 3
p	8	3 7 33	+0 16 2	λ	8	2 52 24	−4 42 7
q	9	3 6 30	−0 51 4	μ	8–9	2 50 26	−5 51 5
r	8–9	3 6 23	−3 22 7	ν	7	2 48 22	−5 50 4
s	5–6	3 6 19	−1 39 9	χ	7–8	2 46 58	−5 45 7
t	8–9	3 6 10	−3 28 3				

Date.	Star of Comparison.	Mag.	A. R. of M. P. h m s	Decl. of M. P. ° ′	4 Hours East. Position-Angle. ° ′	4 Hours East. Distance. ′	4 Hours West. Position-Angle. ° ′	4 Hours West. Distance. ′
Oct. 10	*b*	8–9	3 17 3	+1 28	80 47	31·9	67 36	33·5
	g	7–8	3 14 9	+1 13	240 0	49·0	244 30	45·8
Oct. 11	*c*	8–9	3 17 0	+1 3	121 20	36·0	123 43	35·5
	g	7–8	3 14 8	+1 6	253 50	41·5	259 29	39·3
Oct. 12	*c*	8–9	3 17 1	+0 56	99 49	35·0	92 49	36·0
	g	7–8	3 14 7	+1 0	272 21	36·0	279 2	35·7
Oct. 13	*d*	7	3 16 4	+0 37	132 23	28·8	123 25	27·0
	h	7–8	3 14 5	+0 45	264 41	36·5	272 35	35·1
	It is preferable to use *c* instead of *d* if height enough.							
	c	8–9	3 16 9	+0 50	79 55	38·9	73 44	41·2
Oct. 14	*d*	7	3 16 2	+0 30	103 55	26·2	93 1	26·8
	h	7–8	3 14 3	+0 38	286 52	33·5	294 43	33·9
Oct. 15	*d*	7	3 16 1	+0 24	76 59	30·0	70 7	33·5
	h	7–8	3 14 2	+0 32	309 6	36·4	316 4	38·0

Date.	Star of Comparison.	Mag.	A. R. of M. P.	Decl. of M. P.	4 Hours East. Position-Angle.	4 Hours East. Distance.	4 Hours West. Position-Angle.	4 Hours West. Distance.
			h m s	° ′	° ′	″	° ′	
Oct. 16	*a*	7–8	3 17 5	−0 10	103 2	89·9	108 40	90·0
	p	8	3 11 1	+0 11	273 31	110·0	276 19	108·1
Oct. 17	*a*	7–8	3 17 4	−0 16	103 10	91·3	99 55	92·4
	p	8	3 11 0	+0 5	281 6	105·8	283 54	104·5
Oct. 18	*a*	7–8	3 17 2	−0 22	94 36	95·0	91 52	97·0
	p	8	3 10 8	−0 2	288 52	104·1	291 43	103·3
Oct. 19	*a*	7–8	3 17 0	−0 29	86 50	101·0	84 50	103·5
	p	8	3 10 5	−0 8	297 12	103·0	299 31	103·0
Oct. 20	*a*	7–8	3 16 7	−0 35	80 11	108·2	78 20	111·5
	p	8	3 10 3	−0 15	304 29	104·1	307 30	105·0
Oct. 21	*e*	9	3 13 7	−0 58	96 5	30·0	88 5	32·0
	n	9	3 10 9	−0 57	269 8	55·5	273 34	52·5

It is doubtful, however, from moonlight if *n* and *e* can be observed, if so, take

Date.	Star of Comparison.	Mag.	A. R. of M. P.	Decl. of M. P.	4 Hours East. Position-Angle.	4 Hours East. Distance.	4 Hours West. Position-Angle.	4 Hours West. Distance.	
	y	6–8	3 8 9	−0 58	268 58	113·8	271 3	111·0	
	y } * { *η*	6–8, 6–8	3 3 0	−1 38	219 38	104·0	219 38	104·0	
Oct. 22	*k*	5–6	3 12 0	−1 17	199 35	16·5	195 5	11·5	No other star conveniently situated, parallax will depend on position-angle.
Oct. 23	*k*	5–6	3 11 7	−1 23	145 14	4·9	77 35	5·5	
Oct. 24	*k*	5–6	3 11 5	−1 29	48 40	13·9	44 2	18·5	
	s	5–6	3 8 7	−1 38	264 12	74·5	267 58	71·5	
Oct. 25	*f*	8–9	3 12 1	−1 55	111 4	47·2	105 52	48·3	
	s	5–6	3 8 5	−1 44	274 31	67·0	278 38	64·0	

In the event of *f* being too faint to observe in moonlight, use *i*.

Date.	Star of Comparison.	Mag.	A. R. of M. P.	Decl. of M. P.	4 Hours East. Position-Angle.	4 Hours East. Distance.	4 Hours West. Position-Angle.	4 Hours West. Distance.
	i	7–8	3 11 6	−2 32	163 5	96·8	160 24	94·0
Oct. 26	*f*	8–9	3 11 8	−2 1	95 55	52·0	91 36	55·2
	s	5–6	3 8 2	−1 50	286 45	60·2	291 43	59·0

If *f* found too faint, use *i*.

Date.	Star of Comparison.	Mag.	A. R. of M. P.	Decl. of M. P.	4 Hours East. Position-Angle.	4 Hours East. Distance.	4 Hours West. Position-Angle.	4 Hours West. Distance.
	i	7–8	3 11 3	−2 38	155 39	88·3	152 17	86·2
Oct. 27	*i*	7–8	3 11 0	−2 44	146 36	82·7	143 7	81·7
	s	5–6	3 7 9	−1 56	300 16	56·8	305 19	57·0
Oct. 28	*i*	7–8	3 10 7	−2 50	135 24	79·5	132 7	79·0
	a	7–8	3 6 4	−2 16	277 56	77·0	281 35	75·0

* The distance *y*–*η* being nearly that of Juno-*y*, the change as shown by heliometric observation of the distance *y*-*η*, will probably be very nearly the correction to apply to observation of Juno with *y*.

Date.	Star of Com-parison.	Mag.	A. R. of M. P.	Decl. of M. P.	4 Hours East. Position-Angle.	4 Hours East. Distance.	4 Hours West. Position-Angle.	4 Hours West. Distance.	
			h m s	° ′	° ′	″	° ′	″	
Oct. 29	*i*	7–8	3 10 4	−2 56	125 2	78·5	121 19	79·2	Measure first *i* & *a* next on if sufficiently bright, then *o* if there is time.
	a	7–8	3 6 0	−2 22	287 46	70·5	291 51	69·2	
	m	8–9	3 8 6	−2 36	121 21	12·0	98 45	14·0	
	o	7–8	3 8 2	−2 41	188 22	16·2	173 14	12·5	
Oct. 30	*o*	7–8	3 7 9	−2 47	121 0	9·5	93 20	10·7	*o* being so near the Planet, it is desirable to measure it as well as the opposite stars *i* and *a*.
	i	7–8	3 10 1	−3 2	114 23	81·1	111 18	81·0	
	a	7–8	3 5 7	−2 28	300 0	67·0	304 12	66·3	
Oct. 31	*o*	7–8	3 7 6	−2 52	68 51	17·9	63 13	22·5	
	x	8–9	3 6 3	−2 56	268 13	21·0	279 15	18·3	
	Should the star *x* be found too faint, use								
	i	7–8	3 9 8	−3 7	105 19	86·4	102 43	88·6	
	η	6–8	3 3 9	−2 37	291 32	102·3	294 13	100·3	
Nov. 1	*r*	8–9	3 6 3	−3 15	180 0	17·2	165 50	14·0	
	x	8–9	3 6 0	−3 1	314 7	15·0	332 16	15·8	
	Should these stars be found too faint, use								
	i	7–8	3 9 4	−3 12	97 14	94·8	94 54	97·0	
	η	6–8	3 3 5	−2 42	300 0	97·2	302 43	96·2	
Nov. 2	*t*	8–9	3 5 9	−3 23	155 6	14·0	132 54	13·0	
	x	8–9	3 5 7	−3 6	357 4	20·5	6 19	24·2	
	Take also, if possible,								
	o	7–8	3 6 9	−3 3	53 17	46·0	52 12	51·3	Near *γ* is *δ* (Mag. 9) a little farther from *o*; do not observe this.
	γ	8	3 4 1	−3 43	222 6	69·0	221 49	63·9	
Nov. 3	*r*	8–9	3 5 6	−3 25	81 12	20·0	74 13	25·0	Care must be taken not to observe the rather brighter star *t* near *r*.
	β	9	3 4 0	−3 35	241 18	35·0	244 30	29·0	
	If *r* and *β* are too faint, observe								
	o	7–8	3 6 5	−3 8	51 10	60·2	43 41	66·0	
	γ	8	3 3 7	−3 48	221 32	54·5	220 56	49·0	
Nov. 4	*t*	8–9	3 5 2	−3 33	74 2	27·5	70 25	31·7	
	β	9	3 3 5	−3 40	252 13	20·7	258 25	16·6	
	Observe also								
	x	8–9	3 4 9	−3 16	25 29	45·6	27 58	50·0	
	γ	8	3 3 4	−3 53	218 55	40·1	218 25	35·6	
Nov. 5	*z*	6–7	3 4 3	−4 2	147 41	36·0	130 51	36·3	
	β	9	3 3 3	−3 45	290 32	9·8	317 45	8·9	

If *β* is too faint, observe

Date.	Star of Comparison.	Mag.	A. R. of M. P.			Decl. of M. P.		4 Hours East. Position-Angle.		4 Hours East. Distance.	4 Hours West. Position-Angle.		4 Hours West. Distance.	
			h	m	s	°	′	°	′		°	′		
	γ	8	3	3	1	−3	58	216	4	26·0	214	4	21·5	Care must be taken not to observe the rather brighter star γ near δ.
Nov. 6	β	9	3	3	1	−3	50	4	37	13·5	17	31	17·4	
	δ	9	3	2	6	−4	3	212	45	17·0	215	36	12·6	
			If β and δ are too faint, observe											
	α	6–7	3	3	9	−4	7	125	28	37·0	117	43	39·0	
	η	6–8	3	1	6	−3	7	341	16	104·0	343	41	106·0	
Nov. 7	δ	9	3	2	3	−4	8	160	4	5·3	113	0	5·7	
	ε	9	3	2	3	−4	5	279	13	3·0	9	15	3·1	
			If δ and ε are too faint, observe											
	α	6–7	3	3	6	−4	11	106	16	42·2	102	10	45·2	
	θ	8	3	0	0	−4	28	234	58	79·5	236	50	74·4	
Nov. 8	α	6–7	3	3	2	−4	16	94	37	51·1	90	2	55·0	
	θ	8	2	59	7	−4	32	237	9	66·0	239	15	60·8	
Nov. 9	α	6–7	3	2	9	−4	20	85	22	61·9	83	41	65·8	
	θ	8	2	59	3	−4	36	238	59	52·5	239	6	48·0	
Nov. 10	α	6–7	3	2	5	−4	25	80	12	73·3	78	27	77·2	
	κ	8–9	2	56	9	−4	21	281	34	98·0	283	36	95·0	
Nov. 11	α	6–7	3	2	2	−4	29	75	29	87·0	74	48	90·1	
	λ	8	2	55	8	−4	41	267	42	107·5	268	52	102·8	
Nov. 12	θ	8	2	58	2	−4	48	75	30	13·8	83	58	10·0	
Nov. 13	θ	8	2	57	9	−4	51	318	43	4·5	4	37	5·8	
Nov. 14	ζ	8–9	2	59	1	−5	26	133	38	76·5	130	42	77·6	
	λ	8	2	54	9	−4	51	282	25	77·0	284	31	74·0	
Nov. 15	ζ	8–9	2	58	6	−5	30	125	6	80·0	122	42	81·5	
	λ	8	2	54	5	−4	55	289	27	68·8	291	46	67·0	
Nov. 16	ζ	8–9	2	58	1	−5	33	118	22	85·0	115	48	87·5	
	λ	8	2	54	2	−4	58	297	12	62·5	300	40	60·5	
Nov. 17	ι	7	2	56	6	−5	31	126	40	46·4	122	41	47·4	
	λ	8	2	53	7	−5	0	308	23	56·6	310	19	55·6	
Nov. 18	θ	8	2	56	3	−5	6	54	33	58·4	55	36	62·3	
	μ	8–9	2	52	5	−5	37	249	1	72·2	249	1	67·7	
Nov. 19	θ	8	2	56	0	−5	9	56	38	69·4	57	15	72·5	
	μ	8–9	2	52	2	−5	49	249	1	61·2	249	1	57·3	
Nov. 20	ι	7	2	55	6	−5	38	101	1	67·3	99	51	69·7	
	ν	7	2	50	9	−5	41	256	17	80·2	257	6	77·0	
Nov. 21	ν	7	2	55	3	−5	49	96	55	75·7	95	50	77·6	
	ν	7	2	50	6	−5	42	257	49	70·5	258	25	66·8	

Date.	Star of Comparison.	Mag.	A.R. of M.P.			Decl. of M.P.	4 Hours East. Position-Angle.	4 Hours East. Distance.	4 Hours West. Position-Angle.	4 Hours West. Distance.
			h	m	s	° ′	° ′	′	° ′	
Nov. 22	ι	7	2	55	0	−5 42	93 55	83·7	92 18	86·9
	χ	7–8	2	49	6	−5 43	265 24	81·2	266 23	78·0
Nov. 23	ι	7	2	54	7	−5 43	90 28	91·3	89 49	94·2
	χ	7–8	2	49	3	−5 44	267 8	73·0	268 34	70·0
Nov. 24	ι	7	2	54	4	−5 45	89 15	100·0	88 48	103·1
	χ	7–8	2	49	0	−5 46	270 0	64·5	270 24	61·3
Nov. 25	μ	8–9	2	50	5	−5 50	218 39	5·7	187 11	3·4
Nov. 26	μ	8–9	2	50	3	−5 51	115 2	4·6	98 6	8·0
Nov. 27	μ	8–9	2	50	0	−5 52	90 52	11·8	90 0	14·3
	ν	7	2	49	0	−5 51	272 25	20·5	274 25	17·8
Nov. 28	μ	8–9	2	49	8	−5 52	87 33	19·7	85 31	21·3
	χ	7–8	2	49	0	−5 49	281 25	33·9	283 0	32·6
Nov. 29	ν	7	2	48	8	−5 52	299 52	6·3	315 48	5·2
Nov. 30	ν	7	2	48	3	−5 52	11 22	4·1	36 4	4·7

The places of the stars of comparison have been selected from the *Catalogues of the Berlin Equatoreal Star Charts*, for 2^h and 3^h A.R. by Morstadt and d'Arrest respectively, and reduced to 1874.

The A.R. and Decl. of the middle point between the planet and star (M.P.) is given for midnight at Mauritius, the position-angle and distance both for 4^h E. and for 4^h W. of the meridian at Mauritius. The places of the planet are those of the *Nautical Almanac* (Hind's Elements) reduced to the longitude of Mauritius.

Effect of Error in the Tabular Place of Venus, as deduced from Observations near the Ascending Node in 1872 *and* 1873, *at the Royal Observatory, Greenwich, on the time of Ingress and Egress,* 1874, *December* 8. By W. H. M. Christie, Esq.

The observed errors of Geocentric Longitude and E.P.D. must first be reduced to errors of Heliocentric Longitude, and E.P.D. by multiplying by the appropriate factors; then, on the assumption that the Heliocentric Errors at the Transit of 1874, December 8, will be the same as at the passages through the ascending node in 1872 and 1873, the errors of Heliocentric Longitude and E.P.D. previously found are to be converted into errors of Geocentric Longitude and E.P.D. for the Transit by multiplying them by the factors − 2·725 and + 2·725, respectively.

The effect of these errors in altering the distance between the centres of the Sun and *Venus*, at ingress and egress, will be found by resolving along the corresponding radii of the Sun.

The position-angles for Ingress and Egress measured from the N. point towards the E. are 45° and 345° respectively, and the angle at the Sun between the Hour Circle and the Longitude Circle is 5°; hence the position-angles from the longitude circle for ingress and egress are 40° and 340° respectively, and the corresponding factors are:

	Ingress	Egress
Factor for error of Long.	+ ·643	+ ·342
„ „ E.P.D.	− ·766	+ ·940

the positive sign indicating that the tabular time of Ingress or Egress is too late.

The following table gives the tabular errors for the Transit of *Venus*, 1874, December 8, resulting from the observations near the ascending node in 1872 and 1873:

Date.	No. of Obs.	Error of Geocentric Longitude.	Error of Geocentric E.P.D.	Error of Heliocentric Longitude.	Error of Heliocentric E.P.D.
		″	″	″	″
1872 June 28 . . .	3	+2·08	+1·47	+5·03	+3·52
1873 Jan. 18 . . .	7	+1·46	+0·58	+5·69	+0·79
1873 Sept. 14 . . .	4	+1·46	+0·36	+4·58	+0·58

Ingress. Alteration of distance of centres from error of Long.	Ingress. Alteration of distance of centres from error of E.P.D.	Ingress. Total alteration.	Resulting error in tabular time of Ingress.	Egress. Alteration of distance of centres from error of Long.	Egress. Alteration of distance of centres from error of E.P.D.	Egress. Total alteration.	Resulting error in tabular time of Egress.
″	″	″	m	″	″	″	m
−8·75	−7·34	−16·09	−7·4	−4·65	+9·00	+4·35	+2·0
−9·97	−1·65	−11·62	−5·3	−5·30	+2·03	−3·27	−1·5
−8·08	−1·19	− 9·27	−4·2	−4·29	+1·46	−2·83	−1·3

It is to be remarked that the numbers in the three lines of figures under the heads Ingress and Egress exhibit the errors which will be found according as we rely, for the correction of the Tabular Errors in the place of Venus, exclusively on the observations about 1872, June 28, or on those about 1873, January 18, or on those about 1873, September 14.

Royal Observatory, Greenwich,
1874, *April* 22.

Corrigendum in the "Results of Astronomical Observations at the Cape of Good Hope." Appendix D. p. 449. By Captain J. Herschel, R.E.

The subject of this correction is, the place of one of the Red Stars of which App. D contains a list. It is there entered as follows:

"R.A. $21^{h}\ 37^{m}\ 19^{s}{\cdot}7$ N.P.D. $52^{\circ}\ 54'\ 47''$ Mag. 8.

"Description: Most beautiful and extremely intense; ruby colour. Two obs."

The Sweep in which the observation supplying the above place occurs is No. 188, and was made in October 1827: the second observation has not been found.

There is, however, good reason to believe that the observation should have stood as

R.A. $21^{h}\ 36^{m}\ 19^{s}{\cdot}7$ N.P.D. $52^{\circ}\ 44'\ 47''$.

The evidence is curious enough to be worth giving at length.

Writing on another subject, Prof. Argelander took occasion to request me to examine the original record, as he suspected the above errors. I did so, and assured him that there was no error of reduction or transcription. It happens, however, that the "Ruby Stars" R.A., is *inferred* from the note, "it is R.A. 7 beats preceding the 1st of the two stars before." These Prof. Argelander identifies, beyond doubt, as Lalande 42403* (or 79 Cygni) and 42406, the first of which, 79 Cygni, has R.A. (1830) $21^{h}\ 36^{m}\ 24^{s}{\cdot}0$. Consequently, the R.A. of the "Ruby Star" should be $21^{h}\ 36^{m}\ 20^{s}{\cdot}5$.

That these two stars should have been noted down at all was accidental, as appears from the remark—"two stars, 7·8 and 8^{m} involved in a *v.* L. milky nebula, 6′ diam. *p* B" and "? dew, on looking again, no nebula." Their entry, however, proves the error, in all three, of 1^{m} in R.A.; while the distraction of the attention (which may be presumed) renders probable the further mistake of 10′ in N.P.D., without which the identity of the Red Star is not determined.

The above corrections are communicated at Prof. Argelander's request.

Collingwood,
1874, *April* 9.

Captain Herschel, in a letter dated 13th April, adds: "I had read nothing about this star in the *Monthly Notices* when I wrote the Note. I now find that it is said to have been seen, in two instances, in the place where it ought not to be! There is *no doubt* about the error in R.A.—and there is no *evidence* of error in N.P.D. Are there then *two* Red Stars, 1^{m} apart in R.A. and 10′ in N.P.D.? It is very curious.

* This seems to be a mistake: the N.P.D. of 42403 differs by 5′ from that of 79 Cygni, in the edition I have.

On the Number of Distinct Terms in a Symmetrical or Partially Symmetrical Determinant. By Prof. Cayley.

The determination of a set of unknown quantities by the method of least squares is effected by means of formulæ depending on symmetrical or partially symmetrical determinants; and it is interesting to have an expression for the number of distinct terms in such a determinant.

The terms of a determinant are represented as duads, and the determinant itself as a bicolumn; viz. we write, for instance,

$$\left\{\begin{matrix} aa \\ bb \\ pp' \\ qq' \end{matrix}\right\} \text{ to represent the determinants } \left|\begin{matrix} aa, & ab, & ap', & aq' \\ ba, & bb, & bp', & bq' \\ pa, & pb, & pp', & pq' \\ qa, & qb, & qp', & qq' \end{matrix}\right|$$

This being so if the duads are such that in general $rs = sr$, then the determinant is wholly or partially symmetrical; viz., the determinant just written down, for which the bicolumn contains such symbols as pp' and qq', (each letter $p, q \ldots$ being distinct from every letter $p', q', \ldots$) is partially symmetrical, but a determinant such as $\left\{\begin{matrix} a\,a \\ b\,b \\ c\,c \end{matrix}\right\}$ is wholly symmetrical. A determinant for which the bicolumn has m rows aa, bb, &c., and n rows pp', qq', &c. is called a determinant (m, n); and the number of distinct terms in the developed expression of the determinant is taken to be $\phi\,(m, n)$; the problem is to find the number of distinct terms $\phi\,(m, n)$.

Consider a determinant (m, n) where n is not $= 0$; for instance, the determinant above written down, which is $(2, 2)$; this contains terms multiplied by qa, qb, qp', qq' respectively: where, disregarding signs, the whole factor multiplied by qa is $\left\{\begin{matrix} b\,b \\ a\,p' \\ p\,q' \end{matrix}\right\}$, which is a determinant $(1, 2)$, and similarly the whole factor multiplied by qb is a determinant $(1, 2)$. But the whole factor multiplied by qp' is the determinant $\left\{\begin{matrix} a\,a \\ b\,b \\ pq' \end{matrix}\right\}$, which is a determinant $(2, 1)$, and the whole factor multiplied by qq' is also a determinant $(2, 1)$.

Hence, observing that qa, qb, qp', qq' are distinct terms occurring *only* in the last line of the determinant, the number of distinct terms is equal to the sum of the numbers of distinct terms in the several component parts, or we have $\phi\,(2, 2) = 2\,\phi\,(1, 2) + 2\,\phi\,(2, 1)$, and so in general:

$$\phi\,(m, n) = m\,\phi\,(m-1, n) + n\,\phi\,(m, n-1).$$

Consider next a completely symmetrical determinant $(m, 0)$; for instance $(4, 0)$, the determinant

$$\left\{\begin{matrix} aa \\ bb \\ cc \\ dd \end{matrix}\right\}, = \left|\begin{matrix} aa, & ab, & ac, & ad \\ ba, & bb, & bc, & bd \\ ca, & cb, & cc, & cd \\ da, & db, & dc, & dd \end{matrix}\right|$$

We have *first* the terms containing dd; the whole factor is $\left\{\begin{matrix} a\,a \\ b\,b \\ c\,c \end{matrix}\right\}$, which is a determinant $(3, 0)$; *secondly*, the terms containing $ad.da$, or the like combinations, $bd.db$ or $cd.dc$; the whole factor multiplied by $ad.da$ is $\left\{\begin{matrix} a\,a \\ b\,b \end{matrix}\right\}$, which is a determinant $(2, 0)$; *thirdly*, the terms containing $ad.db + bd.da$, $= 2\ ad.bd$; or the like combinations $2\ ad.cd$ or $2\,bd.cd$: the whole factor multiplying the term $2\,ad.bd$ is $\left\{\begin{matrix} c\,c \\ b\,a \end{matrix}\right\}$, which is a determinant $(1, 1)$. Hence observing that ad, bd, cd, $=$ da, db, dc, and dd are terms occurring *only* in the last line and column of the original determinant, it is clear that the number of distinct terms in the original determinant is equal to the sum of the numbers of distinct terms in the component parts, or that we have $\phi(4, 0) = \phi(3, 0) + 3\,\phi(2, 0) + 3\,\phi(1, 1)$; and so in general:

$$\phi(m, 0) = \phi(m-1, 0) + m\,\phi(m-2, 0) + \frac{m \,.\, m-1}{2}\,\phi(m-3, 1).$$

The two equations of differences, together with the initial values $\phi(0, 0) = 1$, $\phi(1, 0) = \phi(0, 1) = 1$, $\phi(2, 0) = \phi(1, 1) = \phi(1, 2) = 2$ enable the calculation of the successive values of $\phi(m, n)$: viz., arranging these in the order

$$\begin{matrix} \phi(0, 0) \\ \phi(1, 0),\quad \phi(0, 1) \\ \phi(2, 0),\quad \phi(1, 1),\quad \phi(0, 2) \\ \phi(3, 0), \text{ \&c., \&c.} \end{matrix}$$

we calculate simultaneously the lines $\phi(m, 0)$, $\phi(m, 1)$; and thence successively the remaining lines $\phi(m, 2)$, $\phi(m, 3)$, &c.: the values up to $m + n = 6$ being in fact

$$\begin{matrix} 1 \\ 1,\ \ 1 \\ 2,\ \ 2,\ \ 2 \\ 5,\ \ 6,\ \ 6,\ \ 6 \\ 17,\ \ 23,\ \ 24,\ \ 24,\ \ 24 \\ 73,\ \ 109,\ \ 118,\ \ 120,\ \ 120,\ \ 120 \\ 388,\ \ 618,\ \ 690,\ \ 714,\ \ 720,\ \ 720,\ \ 720 \end{matrix}$$

where the process for the first two lines is

$$\begin{array}{rl} 5 = & 2 + {}_2 .\ 1 + \quad .\ 1 \\ 17 = & 5 + {}_3 .\ 2 + {}_3 .\ 2 \\ 73 = & 17 + {}_4 .\ 5 + {}_6 .\ 6 \\ 388 = & 73 + {}_5 .17 + \quad .23 \\ \vdots & \end{array} \qquad \begin{array}{rl} 6 = & {}_2 .\ \ 2 + \ 2 \\ 23 = & {}_3 .\ \ 6 + \ 5 \\ 109 = & {}_4 .\ 23 + 17 \\ 618 = & {}_5 .109 + 23 \\ \vdots & \end{array}$$

the larger figures being those of the two lines, and the smaller ones numerical multipliers. And then for the third line, fourth line, &c., we have

$$\begin{array}{rl} 6 = & {}_1 .\ \ 2 + {}_2 .\ \ 2 \\ 24 = & {}_2 .\ \ 6 + {}_2 .\ \ 6 \\ 118 = & {}_3 .\ 24 + {}_2 .\ 23 \\ 690 = & {}_4 .118 + {}_2 .109 \end{array} \qquad \begin{array}{rl} 120 = & {}_2 .\ \ 24 + {}_3 .\ 24 \\ 714 = & {}_3 .120 + {}_4 .118 \\ \vdots & \end{array}$$

and so on.

This is, in fact, the easiest way of obtaining the actual numerical values; but we may obtain an analytical formula. Considering the two equations

$$\phi(m, 1) = m\,\phi(m-1, 1) + \phi(m, 0),$$

$$\phi(m, 0) = \phi(m-1, 0) + m\,\phi(m-2, 0) + \frac{m\,.\,m-1}{2}\,\phi(m-3, 1);$$

and using the first of these to eliminate the term $\phi(m-3, 1)$ and resulting terms $\phi(m-4, 1)$, &c. which present themselves in the second equation, this, after a succession of reductions, becomes

$$\begin{aligned} \phi(m, 0) = {} & \phi(m-1, 0) \\ & + (m-1)\,\phi(m-2, 0) \\ & + \frac{m\,.\,m-1}{2}\left\{ \begin{aligned} & \phi(m-3, 0) \\ & + (m-3)\,\phi(m-4, 0) \\ & \quad \vdots \\ & + (m-3)\ldots 3\,.\,2\,\phi(1, 0) \\ & + (m-3)\ldots 3\,.\,2\,.\,1 \end{aligned} \right\} \end{aligned}$$

or, observing that the last term $(m-3)\ldots 3\,.\,2\,.\,1$ is, in fact, $= (m-3)\ldots 3\,.\,2\,,\,1\,\phi(0, 0)$, this may be written:

$$\begin{aligned} 2\,\phi(m, 0) - \phi(m-1, 0) - (m-1)\,\phi(m-2, 0) = {} & \phi(m-1, 0) \\ & + (m-1)\,\phi(m-2, 0) \\ & + (m-1)(m-2)\,\phi(m-3, 0) \\ & \quad \vdots \\ & + (m-1)\ldots 3\,.\,2\,.\,1\,\phi(0, 0). \end{aligned}$$

And hence assuming

$$u = \phi(0,0) + \frac{x}{1}\phi(1,0) + \frac{x^2}{1\,.\,2}\phi(2,0)\,.\,.\,. + \frac{x^m}{1\,.\,2\,.\,.\,.\,m}\phi(m,0) + \,.\,.\,.$$

we find at once

$$2\frac{du}{dx} - u - xu = \frac{u}{1-x}.$$

that is

$$2\frac{du}{u} = dx\left(1 + x + \frac{1}{1-x}\right),$$

or integrating and determining the constant so that for $x = 0$ u shall become $= 1$, we have

$$u = \frac{e^{\frac{1}{2}x+\frac{1}{4}x^2}}{\sqrt{1-x}};$$

wherefore we have

$$\phi(m,0) = 1\,.\,2\,.\,.\,.\,m \text{ coefft. } x^m \text{ in } \frac{e^{\frac{1}{2}x+\frac{1}{4}x^2}}{\sqrt{1-x}}$$

Developing as far as x^6, the numerical process is

1	$\frac{1}{2}$	$\frac{1}{8}$	$\frac{1}{48}$	$\frac{1}{384}$	$\frac{1}{3840}$	$\frac{1}{46080}$
1		$\frac{1}{4}$		$\frac{1}{32}$		$\frac{1}{384}$
1	$\frac{1}{2}$	$\frac{1}{8}$	$\frac{1}{48}$	$\frac{1}{384}$	$\frac{1}{3840}$	$\frac{1}{46080}$
		$\frac{1}{4}$	$\frac{1}{8}$	$\frac{1}{32}$	$\frac{1}{192}$	$\frac{1}{1536}$
				$\frac{1}{32}$	$\frac{1}{64}$	$\frac{1}{256}$
						$\frac{1}{384}$

1	$\frac{1}{2}$	$\frac{3}{8}$	$\frac{7}{48}$	$\frac{25}{384}$	$\frac{27}{1280}$	$\frac{331}{46080}$
1	$\frac{1}{2}$	$\frac{3}{8}$	$\frac{5}{16}$	$\frac{35}{128}$	$\frac{63}{256}$	$\frac{231}{1024}$

1	$\frac{1}{2}$	$\frac{3}{8}$	$\frac{7}{48}$	$\frac{25}{384}$	$\frac{27}{1280}$	$\frac{331}{46080}$
	$\frac{1}{2}$	$\frac{1}{4}$	$\frac{3}{16}$	$\frac{7}{96}$	$\frac{25}{768}$	$\frac{27}{2560}$
		$\frac{3}{8}$	$\frac{3}{16}$	$\frac{9}{64}$	$\frac{7}{128}$	$\frac{25}{1024}$
			$\frac{5}{16}$	$\frac{5}{32}$	$\frac{15}{128}$	$\frac{35}{768}$
				$\frac{35}{128}$	$\frac{35}{256}$	$\frac{105}{1024}$
					$\frac{63}{256}$	$\frac{63}{512}$
						$\frac{231}{1024}$

	1	1	1	$\frac{5}{6}$	$\frac{17}{24}$	$\frac{73}{120}$	$\frac{97}{180}$
× by	1	1	2	6	24	120	720
	1	1	2	5	17	73	388

agreeing with the former values.

The expression of $\phi(m, 0)$ once found, it is easy thence to obtain

$$\phi(m, 1) = 1.2 \ldots . m \text{ coefft. } x^m \text{ in } \frac{e^{\frac{1}{2}x+\frac{1}{4}x^2}}{(1-x)^{\frac{3}{2}}}$$

$$\phi(m, 2) = 1.2 \ldots . m \text{ coefft. } x^m \text{ in } \frac{2e^{\frac{1}{2}x+\frac{1}{4}x^2}}{(1-x)^{\frac{5}{2}}}$$

$$\phi(m, 3) = 1.2 \ldots . m \text{ coefft. } x^m \text{ in } \frac{2.3e^{\quad x+\frac{1}{4}x^2}}{(1-x)^{\frac{7}{2}}}$$

and so on, the law being obvious.

Observations of the Eclipses of Jupiter's Satellites made at the Observatory of Toulouse, 1874.

(Communicated by the Astronomer Royal.)

	1874.	Satellite.	Phenomenon.	Observer.	Instrument.	Toulouse M.T. h m s	Mean Time from N.A. h m s	Corr. to N.A. m s
Jan.	4	I.	Disapp.	T	A	17 0 50·1	16 54 27·7	+0 32·8
	13	I.	„	T	A	13 22 7·6	13 15 49·7	+0 28·3
	13	I.	„	P	B	13 22 3·8	13 15 49·7	+0 24·5
	13	IV.	„	T	A	18 34 23·9	18 32 17·3	−3 43·0
	* 23	III.	Reapp.	T	A	13 55 10·6	13 44 33·0	+4 48·0
	25	II.	Disapp.	P	B	16 44 41·9	16 37 57·8	+0 54·5
	25	II.	„	T	A	16 44 41·0	16 37 57·8	+0 53·6
	29	I.	„	T	A	11 36 40·1	11 30 15·4	+0 35·1
	29	I.	„	P	B	11 36 29·1	11 30 15·4	+0 24·1
	30	IV.	„	P	B	12 33 56·4	12 32 10·0	−4 3·2
	30	IV.	„	T	A	12 33 20·9	12 32 10·0	−4 38·7
	30	III.	„	P	B	14 44 1·6	14 34 38·9	+3 33·1
	30	III.	„	T	A	14 44 21·0	14 34 38·9	+3 52·5
	30	IV.	Reapp.	T	A	15 32 33·8	15 24 46·1	+1 58·1
	30	III.	„	T	A	17 51 24·6	17 41 1·7	+4 33·3
Feb.	5	I.	Disapp.	T	A	13 29 38·3	13 23 25·0	+0 23·7
	5	I.	„	P	B	13 29 43·8	13 23 25·0	+0 29·2
	* 14	I.	„	T	A	9 50 40·5	9 44 57·4	−0 6·5
	* 14	I.	„	P	B	9 50 44·0	9 44 57·4	−0 3·0
	19	II.	„	T	A	13 50 20·0	13 43 28·8	+1 1·6
	19	II.	„	P	B	13 50 18·6	13 43 28·8	+1 0·2
	19	I.	„	T	A	17 15 59·7	17 9 56·7	+0 13·4
	19	I.	„	P	B	17 16 10·7	17 9 56·7	+0 24·4
	21	I.	„	T	A	11 44 31·5	11 38 16·7	+0 25·2
	21	I.	„	P	B	11 44 39·3	11 31 16·7	+0 33·0
	* 26	II.	„	P	B	16 25 7·9	16 19 39·2	−0 20·9
	28	I.	„	T	A	13 37 36·4	15 31 41·6	+0 5·2
	28	I.	„	P	B	13 37 55·7	13 31 41·6	+0 24·5
Mar.*	2	I.	„	P	B	8 5 52·7	8 0 2·9	+0 0·2
	7	III.	„	P	B	10 33 48·3	10 24 23·0	+3 35·7
	7	I.	„	T	A	15 31 6·8	15 25 13·0	+0 4·2
	7	I.	„	P	B	15 31 14·1	15 25 13·0	+0 11·5
	23	I.	Reapp.	T	A	15 58 33·2	15 52 35·0	+0 8·6

1874.		Satellite.	Phenomenon.	Observer.	Instrument.	Toulouse M.T. h m s	Mean Time from N.A. h m s	Corr. to N.A. m s
Mar.	23	I.	Reapp.	P	B	15 58 5·7	15 52 35·0	−0 18·9
	23	II.	„	T	A	16 9 25·4	16 3 51·8	−0 16·0
	23	II.	„	P	B	16 9 35·7	16 3 51·8	−0 5·7
April	1	I.	„	T	A	12 20 42·2	12 14 54·5	−0 1·9

The observations were made by M. Tisserand, and by M. Perrotin, *Aide-astronome*, whose names are referred to under the initials T and P. The aperture of the telescope employed by M. Tisserand is 11 centimètres, and of that used by M. Perrotin 15 centimètres. The longitude of the Observatory of Toulouse has been assumed to be $5^m\ 49^s{\cdot}6$ East of Greenwich. The observations marked with an asterisk were made through clouds.

Naked-eye Observation of Jupiter's Satellites. By W. F. Denning, F.R.M.S., F.M.S.

(*Communicated by the Rev. T. W. Webb, M.A., F.R.A.S.*)

It may be worth recording that on the night of April 3, at about 10^h I unmistakeably saw Sat. III. and IV. of *Jupiter* with the naked eye. On the occasion referred to, these satellites were particularly well placed for such an observation, being near their greatest elongations (West) from their primary. They were seen steadily and separately several times. I also observed them in the finder (power 5) of my 10¼-inch reflector and in an ordinary opera-glass (power 3); and from the remarkable ease with which they were visible, I was not surprised that unaided vision sufficed to reveal them, though previously I had been sceptical on the point, knowing it to be a disputed, albeit a well-attested one.

My first attempts to discern the satellites were unsuccessful, owing to my having taken insufficient care to cut off the planet's marginal rays, but, having accomplished this, they became perceptible. At the time, the moon was only two days past the full, but quite hidden by a bank of cumulus cloud low down in S.E. Additional weight may be attached to this observation, if I add that on several dark nights during the past winter, I distinguished 13 stars in the Pleiades, and have occasionally seen *Jupiter* in full sunshine (see *Monthly Notices*, vol. xxxiii. p. 179).

In concluding this brief paper, I may just refer to the extraordinary variations in the apparent brightness of *Jupiter's* fourth satellite. I have sometimes seen it faint and ill-defined in my 10¼-inch reflector, and in smaller instruments it has been a very dim object indeed. On March 28, 1873, it was *barely visible at all* to Mr. H. C. Key, using 3 inches of aperture and power 140;

while, on the following evening, it appeared to have regained its ordinary degree of brightness (see *English Mechanic*, vol. xvii. p. 62). In fact, this satellite, though occasionally within the range of the unassisted eye, is also at times so extremely faint as to be hardly perceptible with considerable telescopic power; and there seems little doubt, therefore, that the reflective properties of its surface are very unequally distributed, and that these inequalities become strikingly manifest in certain parts of the satellite's orbit.

Cotham Park, Bristol,
1874, *April* 7.

On the Appearance of Round Bright Spots on Jupiter. By W. Lassell, Esq.

(*Extract from a Letter to Mr. Dunkin.*)

On directing the telescope to *Jupiter* about 11 P.M. on the 23rd ult., several round bright spots almost immediately caught my eye, situated in the principal southern belt, exactly as I first saw them on March 27, 1850, of which a short description and diagram are given in Vol. x. of the *Monthly Notices*, p. 134. The atmosphere is rarely so favourable here as it was on March 23, yet I did not see the phenomena nearly so sharply as on the former occasion I refer to. I believe the appearance of these spots is very rare, as I have not seen them for many years, and the general similarity of the aspect of the planet now and then, suggests the idea that the various phases return in cycles, which I think more probable than that absolute secular changes occur in the heavenly bodies within the limit of time of any human records. I could not obtain any distinct impression of colour on the face of the planet.

Ray Lodge, Maidenhead,
1874, *April* 8.

The Trustees for the Johnson Memorial Prize at Oxford have proposed for the subject of the next Essay: *On the Present State of our Knowledge of the Physical Constitution and Probable Origin of Comets.*

The Essays are to be sent to the Vice-Chancellor on or before March 31, 1875.

Note on Paragraph 2, p. 533. By R. Proctor, Esq.

I have learned, with much regret, that this paragraph has caused pain to Admiralty representatives. Nothing could have been farther from my purpose. I intended, indeed, to indicate some degree of amusement at the different views expressed by Admiral Richards and others in 1869 and 1873. I also considered that their estimates of possibilities had been influenced by "those in authority" (*in authority at Greenwich, and as astronomer, not in authority over the Admiralty*). I referred to the authority of Greenwich on astronomical questions, commenting on a legitimate (though in my opinion mistaken) acceptance of Sir G. Airy's views by Admiralty representatives, and not referring to, or imagining even, any unworthy subservience on their part to authority *of influence or control. Nothing in the whole course of the discussion respecting the Transit pointed in the slightest degree to the latter explanation* (otherwise I should probably have thought it necessary to be more careful in wording my remarks), while *the former, or legitimate reason, which alone I had in my thoughts while writing, was distinctly advanced by Admiral Richards* in a letter which appeared in the *Times* on or about July 28, 1873, explaining why he advocated certain expeditions in 1869, and opposed them in 1873. My paper was written in great haste, and a comparison of the date (November 7) when it appeared, with the date (October 4) when I left England for America, will show how little opportunity I had for correcting it. I regret very much that my carelessness should have occasioned any pain to gentlemen whom I believe to be worthy of all esteem; but I do most earnestly assure them that I did not intend to indicate, nor do I entertain, the slightest doubt that the explanation of their action accords perfectly with the honourable estimation in which they are held. I venture to express my confidence that they will accept this explanation in the spirit in which it is offered.

London,
1874, *May* 7.

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

Vol. XXXIV. *May* 8, 1874. No. 7.

Sir G. B. Airy, K.C.B., Vice-President, in the Chair.

Henry Glanville Barnacle, Esq., 3 Park Terrace, Greenwich;
C. E. Burton, Esq., Rathmael Rectory, Loughlinstown, Co. Dublin;
Joseph Gledhill, Esq., Bermerside Observatory, Halifax;
Richard Johnson, Esq., Trinity College, Dublin;
Wm. James Lancaster, Esq., Colmore Row, Birmingham;
J. W. Nichol, Esq., 81 Princes Street, Edinburgh;
Col. Charles Ratcliffe, Wyddington, Edgbaston, Birmingham;
The Chevalier de Rosaz, 1 Arundel Terrace, Brighton;

were balloted for and duly elected Fellows of the Society.

On the Solution of the Equations in the Method of Least Squares. By J. W. L. Glaisher.

§ 1. In No. 404 (July 2, 1840) of the *Astronomische Nachrichten*, Bessel published some very elegant formulæ for the solution of the final equations in the method of least squares and the determination of the weights, when the number of unknowns amounted only to three, that had been communicated to him by Jacobi; and in a recent number of the same journal (No. 1960, October 8, 1873) Dr. H. Seeliger of the Leipzig Observatory, has inserted a proof of them. As, however, Dr. Seeliger has confined himself to merely verifying the formulæ, without referring to the points on which their special excellence and characteristic properties depend, I have thought it would be worth while to examine them from this point of view, omitting the proof, which presents no difficulty. And as being very closely connected with this investigation, I have added several

notes—forming the larger portion of the paper—which were suggested by Gauss's original memoirs referred to more particularly further on, and Encke's excellent *résumé* of the whole subject in the *Berliner Astronomische Jahrbücher* for 1834, 1835, and 1836.

§ 2. Writing the equations—

$$\begin{aligned}(aa)\,x + (ab)\,y + (ac)\,z &= (an)\\ (ab)\,x + (bb)\,y + (bc)\,z &= (bn)\\ (ac)\,x + (bc)\,y + (cc)\,z &= (cn)\end{aligned}.$$

Jacobi's formulæ as given in No. 404 of the *Astr. Nach.* are—

$$r = \pm\sqrt{\pm\,(bc)\,(ac)\,(ab)}$$

$$\alpha = \frac{r}{(bc)},\qquad \beta = \frac{r}{(ac)},\qquad \gamma = \frac{r}{(ab)}$$

$$\mathrm{A} = (aa) \mp \alpha\alpha,\qquad \mathrm{B} = (bb) \mp \beta\beta,\qquad \mathrm{C} = (cc) \mp \gamma\gamma$$

$$\alpha' = \frac{\alpha}{\mathrm{A}},\qquad \beta' = \frac{\beta}{\mathrm{B}},\qquad \gamma' = \frac{\gamma}{\mathrm{C}}$$

$$\mathrm{R} = 1 \pm (\alpha\alpha' + \beta\beta' + \gamma\gamma')$$

$$a = \frac{(an)}{\mathrm{R}},\qquad b = \frac{(bn)}{\mathrm{R}},\qquad c = \frac{(cn)}{\mathrm{R}}$$

$$\rho = \alpha' a + \beta' b + \gamma' c$$

$$x = \frac{(an)}{\mathrm{A}} \mp \alpha'\rho,\qquad y = \frac{(bn)}{\mathrm{B}} \mp \beta'\rho,\qquad z = \frac{(cn)}{\mathrm{C}} \mp \gamma'\rho,$$

$$\mathrm{X} = \frac{\mathrm{R\,A}}{\mathrm{R} \mp \alpha\alpha'},\qquad \mathrm{Y} = \frac{\mathrm{R\,B}}{\mathrm{R} \mp \beta\beta'},\qquad \mathrm{Z} = \frac{\mathrm{R\,C}}{\mathrm{R} \mp \gamma\gamma'}$$

$$(vv) = (nn) - \frac{(an)^2}{\mathrm{A}} - \frac{(bn)^2}{\mathrm{B}} - \frac{(cn)^2}{\mathrm{C}} \pm \mathrm{R}\rho\rho\,^*$$

X, Y, Z being the weights, (vv) the sum of the squares of the residuals and the upper or lower signs being taken throughout according to the sign of the product under the radical in r.† Bessel adds, that Jacobi's formulæ give x, y, z and X, Y, Z with only 32 logarithmic entries, while Gauss's method requires 23 entries in order to obtain x, y, z, and 12 more to obtain X, Y, Z: a remark that will be noticed further on.

§ 3. In discussing the formulæ, when the number of un-

* Bessel, by a slip, writes:—

$$(vv) = (nn) - \frac{(an)^2}{\mathrm{A}} - \frac{(bn)^2}{\mathrm{B}} - \frac{(cn)^2}{\mathrm{C}} \pm \frac{\rho\rho}{\mathrm{R}}$$

(*Astr. Nach.*, *No.* 404).

† It may save the reader of Bessel's paper (*Astr. Nach.*, No. 404) a few moments' uncertainty to remark that in the statement of the data of the example there worked out, two of the coefficients are accidentally interchanged—viz., they should be, $(ac) = 28{\cdot}4660$ and $(bc) = 3{\cdot}3000$.

knowns is only three, it is more convenient to write the original equations in their usual mathematical form, viz.:

$$\begin{aligned} ax + hy + gz &= \alpha \\ hx + by + fz &= \beta \\ gx + fy + cz &= \gamma \end{aligned}$$

and to take

$$\nabla = \begin{vmatrix} a, & h, & g, \\ h, & b, & f, \\ g, & f, & c, \end{vmatrix}.$$

Further, let great letters denote the minors of the corresponding small letters, so that

$$\begin{array}{lll} A = bc - f^2, & B = ca - g^2, & C = ab - h^2 \\ F = gh - af, & G = hf - bg, & H = fg - ch \end{array}$$

then

$$\nabla a = BC - F^2, \quad \nabla b = \&c., \&c.; \quad \nabla f = GH - AF, \quad \nabla g = \&c., \&c.$$
$$\nabla x = A\alpha + H\beta + G\gamma, \quad \nabla y = \&c., \&c.$$
$$\nabla \ = Aa + Hh + Gg = \&c.$$

and, for the weights,

$$X = \frac{\nabla}{A}, \qquad Y = \frac{\nabla}{B}, \qquad Z = \frac{\nabla}{C}.$$

It will then be readily seen that the following is a free translation of Jacobi's process, in which the numbers of logarithms and anti-logarithms required are left unaltered; although, for the sake of clearness, certain factors are attached to different quantities from those to which they belong in Jacobi's formulæ, and several signs are changed.

Form the quantities:

$$\log f, \qquad \log g, \qquad \log h$$

$$\frac{F}{f} = \frac{gh}{f} - a, \qquad \frac{G}{g} = \frac{hf}{g} - b, \qquad \frac{H}{h} = \frac{fg}{h} - c,$$

$$\log\left(\frac{F}{f}\right), \qquad \log\left(\frac{G}{g}\right), \qquad \log\left(\frac{H}{h}\right)$$

$$\log F, \qquad \log G, \qquad \log H$$

$$\nabla' = \frac{gh}{F} + \frac{hf}{G} + \frac{fg}{H} - 1, \ \log \nabla'$$

$$\log \alpha, \qquad \log \beta, \qquad \log \gamma$$

$$\rho = \frac{\alpha}{F} + \frac{\beta}{G} + \frac{\gamma}{H}, \ \log \rho$$

$$x = -\frac{\alpha f}{F} + \frac{fgh}{\nabla' F}\rho, \quad y = -\frac{\beta g}{G} + \frac{fgh}{\nabla' G}\rho, \quad z = -\frac{\gamma h}{H} + \frac{fgh}{\nabla' H}\rho$$

$$\log\left(\frac{gh}{F} - \nabla'\right), \qquad \log\left(\frac{hf}{G} - \nabla'\right), \qquad \log\left(\frac{fg}{H} - \nabla'\right)$$

$$X = \frac{\nabla' F}{f\left(\frac{gh}{F} - \nabla'\right)}, \qquad Y = \frac{\nabla' G}{g\left(\frac{hf}{G} - \nabla'\right)}, \qquad Z = \frac{\nabla' H}{h\left(\frac{fg}{H} - \nabla'\right)}$$

$$(vv) = (nn) + \frac{\alpha^2 f}{F} + \frac{\beta^2 g}{G} + \frac{\gamma^2 h}{H} - \frac{fgh}{\nabla'}\rho^2.$$

In order to appreciate what Jacobi effects, it is to be observed that his object clearly was to diminish as much as possible the number of logarithmic entries requisite, at the same time keeping the formulæ symmetrical. Thus he endeavours to replace multiplications and divisions by additions and subtractions, as the latter do not entail reference to a table. If the equations were solved in a straightforward way by calculating A, B, C, F, G, H, and their logarithms, and substituting in the final equations $\nabla x = A\alpha + H\beta + G\gamma$, &c., $X = \frac{\nabla}{A}$, &c., no less than forty-two entries (viz. sixteen logarithms and twenty-six anti-logarithms) would be required for the determination of x, y, z, X, Y, Z. But it is clear that one entry can be dispensed with in the calculation of such a pair of terms as $ab - cd$, by writing them in the form $b\left(a - \frac{cd}{b}\right)$, so that the logarithm of a is not required. Thus, by calculating $\log \frac{F}{f}$, &c., and thence $\log F$, &c., in place of $\log F$, &c., directly, Jacobi avoids the necessity of having to take out the logarithms of a, b, c. The quantity ∇' is connected with ∇ by the relation—

$$\nabla' = \frac{fgh}{FGH}\nabla;$$

it can readily be shown that

$$\nabla = \frac{GH}{f} + \frac{HF}{g} + \frac{FG}{h} - \frac{FGH}{fgh};$$

and the factor $\frac{fgh}{FGH}$ is introduced to replace the calculation of the last term, which would require the taking out of an anti-

logarithm, by the simple subtraction of unity. It will be observed that the calculation of $\frac{gh}{\mathrm{F}} - \nabla'$, &c., requires no tabular entries, as the values of $\frac{gh}{\mathrm{F}}, \frac{hf}{\mathrm{G}}$ and $\frac{fg}{\mathrm{H}}$ were obtained as steps in the calculation of ∇'.

The denominators of X, Y, Z must involve A, B, C as factors, and it is easily shown that

$$\frac{gh}{\mathrm{F}} - \nabla' = \frac{gh}{\mathrm{GH}}\mathrm{A}, \quad \frac{hf}{\mathrm{G}} - \nabla' = \frac{hf}{\mathrm{HF}}\mathrm{B}, \quad \frac{fg}{\mathrm{H}} - \nabla' = \frac{fg}{\mathrm{FG}}\mathrm{C}.$$

As A, B, C, and their logarithms thus have ultimately to be calculated for the determination of the weights, it is natural to enquire whether it would not be more convenient to calculate them at an earlier stage of the process, and use their values in determination of x, y, z. In fact, after finding $\log \nabla'$ it seems as if it would be an improvement for the method to proceed—

$$\log \mu = \log\left(\frac{\mathrm{F}}{f}\right) + \log\left(\frac{\mathrm{G}}{g}\right) + \log\left(\frac{\mathrm{H}}{h}\right), \ \log \nabla = \log \nabla' + \log \mu$$

$$\mathrm{A}' = \frac{gh}{\mathrm{F}} - \nabla', \quad \mathrm{B}' = \frac{hf}{\mathrm{G}} - \nabla', \quad \mathrm{C}' = \frac{fg}{\mathrm{H}} - \nabla'$$

$$\log \mathrm{A} = \log \mu + \log f + \log \mathrm{A}' - \log \mathrm{F}, \log \mathrm{B} = \&\text{c.}, \log \mathrm{C} = \&\text{c.}$$

$$\log \alpha, \quad \log \beta, \quad \log \gamma$$

$$x = \frac{\mathrm{A}\alpha}{\nabla} + \frac{\mathrm{H}\beta}{\nabla} + \frac{\mathrm{G}\gamma}{\nabla}, \ \mathrm{X} = \frac{\nabla}{\mathrm{A}}$$

$$y = \&\text{c.}, z = \&\text{c.}, \mathrm{Y} = \&\text{c.}, \mathrm{Z} = \&\text{c.}$$

By this means we should also save one entry, as with Jacobi $\log \rho$ requires 4 entries, and x, y, z, 6 more, while in the scheme just written, x, y, z, merely require 9 anti-logarithms. Thus, x, y, z, X, Y, Z, are obtained by 31 entries, one less than Jacobi needs; but from one point of view Bessel's comparison is scarcely fair to Jacobi, unless the calculation of $(v\,v)$ is also included. There can be no doubt that the quantity ρ was introduced for the sake of saving entries in the formation of $(v\,v)$, for although one entry more is required in finding x, y, z, two are avoided in calculating $(v\,v)$, which according, to the second scheme, would have to be obtained from—

$$(v\,v) = (n\,n) - \alpha x - \beta y - \gamma z$$

and as the logarithms of x, y, z, still have to be found, 6 entries are required, while Jacobi's method only needs 4. He therefore gains one on the whole process, which requires 36 entries, while the other scheme requires 37. If the values of x, y, z, had been

found by Gauss's method of substitution, the value of (vv) or $(nn.3)$ would need only 3 additional entries, so that no doubt Bessel made the comparison end where he did, as he thus broke off at the point most favourable to Jacobi. But unless the whole process is included, no sufficient reason appears why ρ should have been introduced. On the whole then, Jacobi requires 36 and Gauss 38 entries. In giving the number of additional entries required in finding X, Y, Z, as 12, Bessel was correct; as he refers to § 32 of the *Theoria Combinationis Observationum*, but had he supposed the weights determined in the manner pointed out by Encke, on p. 314 of the *Berl. Astr. Jahrbuch* for 1835 (which is the method reproduced by Chauvenet in his *Astronomy*, vol. ii. p. 537, fourth edition, 1868), only 7 additional entries would be required, and the total number would thus become 33, or 3 less than with Jacobi. The latter, however, has the advantage of symmetry.

§ 3. It is perhaps worth while to here add, for the sake of comparison, the solution of the equations by the method of substitution, with the determination of the weights by Encke's method, using, as far as possible, the same letters as those already employed.

$$\begin{aligned} ax + hy + gz &= \alpha \\ hx + by + fz &= \beta \\ gx + fy + cz &= \gamma \end{aligned}$$

therefore

$$x = \frac{\alpha}{a} - \frac{h}{a}y - \frac{g}{a}z \quad \ldots\ldots\ldots\ldots \quad (1)$$

and

$$y\left(b - \frac{h^2}{a}\right) + z\left(f - \frac{hg}{a}\right) = \beta - \frac{h\alpha}{a};$$

$$y\left(f - \frac{gh}{a}\right) + z\left(c - \frac{g^2}{a}\right) = \gamma - \frac{g\alpha}{a}$$

viz.

$$y\frac{C}{a} - z\frac{F}{a} = \beta'$$

$$-y\frac{F}{a} + z\frac{B}{a} = \gamma'$$

which write

$$\begin{aligned} yC' - zF' &= \beta', \\ -yF' + zB' &= \gamma' \end{aligned}$$

whence

$$y = \frac{F'}{C'}z + \frac{\beta'}{C'}, \quad \ldots\ldots\ldots\ldots \quad (2)$$

and

$$z\left(B' - \frac{F'^2}{C'}\right) = \gamma' + \frac{F'\beta'}{C'} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

(3), (2) and (1) give x, y, z, with 23 entries.

$$Z = B' - \frac{F'^2}{C'} = C'' \text{ say}$$

$$Y = C' . \frac{C''}{B'}$$

$$X = a . \frac{C'}{b} . \frac{C''}{c - \frac{f^2}{b}}$$

$$(v\,v) = (n\,n) - \frac{a^2}{a} - \frac{\beta'^2}{C'} - \frac{\gamma''^2}{C''}, \text{ where } \gamma'' = \gamma' + \frac{F'\beta'}{C'}$$

and the process altogether requires 14 logarithms and 19 antilogarithms.

In the case of two unknowns it can be shown that the method of substitution gives x, y, X, Y, by twelve entries and $(v\,v)$ by two more, while a strictly symmetrical procedure requires thirteen entries for x, y, X, Y, and four more for $(v\,v)$.

§ 4. The geometrical interpretation of the results of the method of least squares, when the number of unknowns amounts to three, possesses some interest.

Construct the quadric

$$(aa)x^2 + (bb)y^2 + (cc)z^2 + 2(bc)yz + 2(ca)zx + 2(ab)xy + 2(an)x + 2(bn)y + 2(cn)z + (nn) = 0 \quad . \quad . \quad . \quad (p)$$

Then the most probable values of x, y, z are the coordinates of its centre; and if the quadric be transformed to the centre as origin, the axes remaining parallel, the equation (p) assumes the form

$$(aa)x^2 + (bb)y^2 + (cc)z^2 + 2(bc)yz + 2(ca)zx + 2(ab)xy + (vv) = 0 \quad . \quad . \quad (q)$$

for, x', y', z', being the coordinates of the centre, $(v\,v) = \Sigma(ax' + by' + cz' + n)^2 = (a\,a)\,x'^2 + \text{\&c.}$ The same thing is also seen to follow at once from the known result $(v\,v) = (a\,n)\,x' + (b\,n)\,y' + (c\,n)\,z' + (n\,n)$.

If in (q) we put $z = 0$, we find the area of the conic so obtained

$$= \pi \frac{(v\,v)}{\sqrt{\{(a\,a)(b\,b) - (a\,b)^2\}}} = \frac{\pi(v\,v)}{\sqrt{\nabla}} \sqrt{Z}$$

so that the square roots of the weights of x, y, z are proportional to the areas of the central sections of (p), formed by planes drawn parallel to the coordinate axes; or what is the same thing, the

probable errors of x, y, z are inversely proportional to the areas of these sections.

Further, since the volume of the quadric $= \frac{4\pi}{3}\sqrt{\frac{(v\,v)^3}{\nabla}}$, it follows that everything else remaining constant except the values of the n's which are (or may be taken to be) the quantities directly observed, then $(v\,v)$, the sum of the squares of the residuals, varies as the superficial dimensions of the quadric; or, what is the same thing, the probable error of an observation varies as the linear dimensions of the quadric.

§ 5. In what follows, I shall adopt entirely the notation that Encke has used in the *Berliner Astronomisches Jahrbuch* for 1835, which is substantially the same as Gauss's; but I shall replace the square brackets, as in $[b\,b\,.\,1]$, by simple parentheses, as in $(b\,b\,.\,1)$, and omit even these in determinants or anywhere else where there is no risk of confusion. The only places that I know of in which Gauss has explained the method of solution of the equations that arise in the method of least squares are §§ 182–186 of the *Theoria Motus* (reprinted, *Werke*, vol. vii. pp. 237–241), the *Theoria Combinationis Observationum* and the *Supplementum* to it (*Werke*, vol. iv.), and the *Disquisitio de Elementis Ellipticis Palladis* (Comm. Soc. Gott., vol. i. 1808–1811), in which his characteristic notation and algorithm first appeared. This memoir has not yet, I believe, been reprinted in his collected works. Although, perhaps, there is not a great deal in Encke's essay that is not implicitly to be found in Gauss, still there is a considerable difference between his full and systematic exposition of the different methods, and the original memoirs in which some parts of the processes are rather sketched than worked out. It is, therefore, convenient to refer generally to the *Jahrbuch* rather than to Gauss's papers, on account of the uniform notation and treatment that characterises the former. The account given by Chauvenet, in his *Astronomy* (vol. ii. pp. 529–549, arts. 41–52, fourth edition), is almost wholly taken from Encke, the chief difference being that the number of unknowns is supposed to be four instead of six.

Writing the final* equations

$$\begin{array}{l}
(a\,a)\,x + (a\,b)\,y + (a\,c)\,z \ldots + (a\,n) = 0 \\
(b\,a)\,x + (b\,b)\,y + (b\,c)\,z \ldots + (b\,n) = 0 \\
(c\,a)\,x + (c\,b)\,y + (c\,c)\,z \ldots + (c\,n) = 0 \\
\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot
\end{array} \qquad \ldots \text{(i)}$$

* These equations are sometimes called the *normal* equations, but the name does not appear very felicitous. The only objection to the word *final* is that Encke terms *Endgleichungen* the equations from which $x, y, z \ldots$ are immediately determined (*Jahrbuch*, 1835, p. 272).

multiplying them by Q, Q′, Q″ . . . and choosing Q, Q′, Q″, . . . so that

$$\begin{aligned}(a\,a)\,Q + (a\,b)\,Q' + (a\,c)\,Q'' + \ldots &= 1\\ (b\,a)\,Q + (b\,b)\,Q' + (b\,c)\,Q'' + \ldots &= 0\\ (c\,a)\,Q + (c\,b)\,Q' + (c\,c)\,Q'' + \ldots &= 0\\ \ldots\ldots\ldots\ldots\ldots\ldots&\end{aligned} \qquad \ldots \text{ ii)}$$

then

$$\begin{aligned}-x &= (a\,n)\,Q + (b\,n)\,Q' + (c\,n)\,Q'' + \ldots\\ &= (a_1\,Q + b_1\,Q' + c_1\,Q'' \ldots)\,n_1 + (a_2\,Q + b_2\,Q' + c_2\,Q'' \ldots)\,n_2 + \ldots\\ &= \alpha_1\,n_1 + \alpha_2\,n_2 + \ldots\end{aligned}$$

and X, the weight of x, $= \dfrac{1}{\alpha_1^2 + \alpha_2^2 + \ldots} = \dfrac{1}{(\alpha\alpha)}$

The value of $\alpha\alpha$ is usually obtained by means of an elegant, though indirect procedure, by which it is shown that $(\alpha\alpha) = Q$, so that the weight of x is found by replacing $(a\,n)$ by -1, and $(b\,n)$, $(c\,n)$, &c. by zeros, in (i); and then solving for x, the value of which is the reciprocal of X. This want of directness is a distinct disadvantage, as the natural course would be to find x, and therefrom calculate $(\alpha\alpha)$. On this point Encke writes: "Die unmittelbare Ableitung der linearen Form würde indessen, wenn jedes α gegeben werden sollte, der Verwickelung der Formeln wegen, theils höchst weitläufig, kaum ausführbar sein, theils auch unnütz, weil zu unserm Zwecke die Summe $[\alpha\,\alpha]$ allein erforderlich ist" (*Jahrbuch*, 1835, p. 289). It is true that α_1, α_2, &c. are not themselves wanted, but it is certainly of interest to exhibit x as a linear function of n_1, n_2, &c., and thence obtain the weights directly, so as to remove the somewhat artificial character that otherwise attaches to the investigation. This is easily effected by the use of determinants, for

$$x\begin{vmatrix} a\,a, & a\,b, & a\,c, & \ldots\\ b\,a, & b\,b, & b\,c, & \ldots\\ c\,a, & c\,b, & c\,c, & \ldots\\ \ldots & \ldots & \ldots & \ldots\end{vmatrix} = \begin{vmatrix} a\,n, & a\,b, & a\,c, & \ldots\\ b\,n, & b\,b, & b\,c, & \ldots\\ c\,n, & c\,b, & c\,c, & \ldots\\ \ldots & \ldots & \ldots & \ldots\end{vmatrix}$$

$$= n_1\begin{vmatrix} a_1, & a\,b, & a\,c, & \ldots\\ b_1, & b\,b, & b\,c, & \ldots\\ c_1, & c\,b, & c\,c, & \ldots\\ \ldots & \ldots & \ldots & \ldots\end{vmatrix} + n_2\begin{vmatrix} a_2, & a\,b, & a\,c & \ldots\\ b_2, & b\,b, & b\,c & \ldots\\ c_2, & c\,b, & c\,c & \ldots\\ \ldots & \ldots & \ldots & \ldots\end{vmatrix} + \&c.$$

since $a\,n$ or $(a\,n) = a_1\,n_1 + a_2\,n_2 + \ldots$

Denoting then, here and henceforth, the symmetrical determinant that multiplies x by ∇, and writing A, B, C, &c. for the minors of $a\,a$, $a\,b$, $a\,c$, . . . in ∇:

$$\nabla^2 (aa) = (a_1 A + b_1 B + c_1 C + \ldots)^2 + (a_2 A + b_2 B + c_2 C + \ldots)^2 + \ldots$$
$$= A\{A(aa) + B(ab) + C(ac) + \ldots\}$$
$$+ B\{A(ab) + B(bb) + C(bc) + \ldots\} + \&c.$$

$$= A \begin{vmatrix} aa, & ab, & ac & \ldots \\ ba, & bb, & bc & \ldots \\ ca, & cb, & cc & \ldots \\ \ldots & \ldots & \ldots & \ldots \end{vmatrix} + B \begin{vmatrix} ab, & ab, & ac & \ldots \\ bb, & bb, & bc & \ldots \\ cb, & cb, & cc & \ldots \\ \ldots & \ldots & \ldots & \ldots \end{vmatrix} + \&c.$$

which expression reduces to its first term, as all the determinants that multiply B, C, D . . . have two columns identical; thus

$$(aa) = \frac{A}{\nabla}$$

and we obtain (aa) in its simplest form, in which it is obviously equal to Q as determined from (ii).

It will have been noticed how well the notation (aa), (ab) . . . for the coefficients is adapted for use in the determinants; it being understood that the small letters $a, b, c, \ldots n$, without suffixes, have no meaning at all except in juxtaposition with one of themselves, we have practically Sylvester's umbral notation for the constituents of a determinant. Thus a and b are supposed unmeaning (except as types of a_1, a_2 . . . and b_1, b_2 . . .), but ab denotes (ab) viz. $a_1 b_1 + a_2 b_2 + \ldots$

The subsidiary quantities $(bb\,.\,1)$, $(bc\,.\,1)\ldots$ $(cc\,.\,2)$, $(cd\,.\,2)\ldots$ &c. (called by Encke *Hülfsgrössen* and by Chauvenet *Auxiliaries*), which occur in the process of solution of the equations by the method of substitution, admit of being expressed very simply as quotients of determinants, and it is interesting to examine what are their actual values in terms of the original coefficients. It is to be observed that the meaning of the notation is not definite until the order in which the letters a, b, c . . . are supposed to follow one another is fixed. Let, therefore, this order be that of the alphabet, so that, e.g.—

$$cd\,.\,1 = cd - \frac{ca\,.\,da}{aa},\quad cd\,.\,2 = cd\,.\,1 - \frac{(cb\,.\,1)(db\,.\,1)}{bb\,.\,1}, \&c.$$

then

$$bb\,.\,1 = bb - \frac{ba\,.\,ba}{aa}$$

so that

$$(aa)(bb\,.\,1) = \begin{vmatrix} aa, & ab \\ ba, & bb \end{vmatrix}$$

Similarly

$$cc\,.\,2 = \frac{1}{bb\,.\,1} \begin{vmatrix} bb\,.\,1, & bc\,.\,1 \\ cb\,.\,1, & cc\,.\,1 \end{vmatrix}$$

$$= \frac{1}{bb.1}\begin{vmatrix} bb - \dfrac{ba.ba}{aa}, & bc - \dfrac{ba.ca}{aa} \\ cb - \dfrac{ca.ba}{aa}, & cc - \dfrac{ca.ca}{aa} \end{vmatrix}$$

$$= \frac{1}{(aa)(bb.1)}\begin{vmatrix} aa, & ab, & ac, \\ ba, & bb, & bc, \\ ca, & cb, & cc, \end{vmatrix}$$

and

$$(aa)(bb.1)(cc.2)(dd.3) = \begin{vmatrix} aa, & ab, & ac, & ad \\ ba, & bb, & bc, & bd \\ ca, & cb, & cc, & cd \\ da, & db, & dc, & dd \end{vmatrix}$$

The general law is now evident for the case when the pair of letters in the auxiliary are identical and correspond to the numeral postfixed: thus $ee.4$ is the quotient of two symmetrical determinants, the denominator being the determinant whose principal diagonal contains aa, bb, cc, dd, and the numerator being the same determinant bordered by an additional row and column, ea, eb, ec, ed, ee. Also, it can be shown in the same way that, for example—

$$(aa)(bb.1)(cc.2)(ef.3) = \begin{vmatrix} aa, & ab, & ac, & af \\ ba, & bb, & bc, & bf \\ ca, & cb, & cc, & cf \\ ea, & eb, & ec, & ef \end{vmatrix}$$

so that every auxiliary involving (say) the postfix 3 has the denominator (aa) $(bb.1)$ $(cc.2)$, viz. the determinant whose principal diagonal is aa, bb, cc; while the numerator is this same determinant, bordered by the constituents formed by uniting the letters involved in the auxiliary with a, b, c, viz. in $ef.3$ the border is af, bf, cf, ef, ec, eb, ea.

The same rule holds good when the letter n is involved, and also when the auxiliary involves a pair of n's; thus

$$nn.2 = \begin{vmatrix} aa, & ab, & an \\ ba, & bb, & bn \\ na, & nb, & nn \end{vmatrix} \div \begin{vmatrix} aa, & ab \\ ba, & bb \end{vmatrix}$$

and the principle of the whole method of formation is clear. We see that the denominator is determined solely by the postfix, and that the pair of letters involved in the auxiliary determines the border, by the addition of which alone the numerator is distinguished from the denominator.

§ 7. In this section it is convenient to suppose with Encke that the number of unknowns amounts to six (x, y, z, w, u, t), although all the reasoning applies equally well whatever the number may be. Both Gauss (*Theoria Motus*, § 182 and *Disq. de Elem. Palladis*, p. 23) and Encke (*Jahrbuch*, p. 273) form the expression for the sum of the squares of the errors, viz.—

$$\Omega = (aa)\,x^2 \ldots + 2\,(ab)\,xy \ldots + 2\,(an)\,x \ldots + (nn)$$

and, with the view of making it a minimum, transform it into

$$\Omega = \frac{AA}{aa} + \frac{B'B'}{bb.1} + \frac{C''C''}{cc.2} + \frac{D'''D'''}{dd.3} + \frac{E^{\text{iv}}E^{\text{iv}}}{ee.4} + \frac{F^{\text{v}}F^{\text{v}}}{ff.5} + nn.6$$

where A is a linear function of all the unknowns, B′ is a linear function from which x is absent, C″ a linear function from which both x and y are absent, &c.

The transformation is practically the same as that employed by Mr. B. Williamson in the Appendix to his *Differential Calculus* (Dublin 1873), and in the *Quarterly Journal of Mathematics* (March 1872), for the determination of the minimum value of a quadratic expression, except that Gauss's and Encke's form is somewhat more general as the linear and constant terms are included. A, B′, C″ . . . are, in fact, *aux facteurs près*, the expressions which, when equated to zero, give the equations from which the unknowns are actually calculated, e.g.—

$$C'' = (cc.2)\,z + (cd.2)\,w + (ce.2)\,u + (cf.2)\,t + (cn.2)$$

that is to say—

$$\begin{vmatrix} aa, & ab \\ ba, & bb, \end{vmatrix} C'' = z \begin{vmatrix} aa, & ab, & ac, \\ ba, & bb, & bc, \\ ca, & cb, & cc, \end{vmatrix} + w \begin{vmatrix} aa, & ab, & ac, \\ ba, & bb, & bc \\ da, & db, & dc \end{vmatrix} + \&c.$$

Gauss's reasoning also shows that the minimum value of Ω is $nn\,.\,6$; and, from what has been said, it follows that

$$nn.6 = \frac{1}{\nabla} \begin{vmatrix} aa, & ab \ldots af, & an \\ ba, & bb \ldots bf, & bn \\ \ldots & \ldots\quad \ldots & \ldots \\ fa, & fb \ldots ff, & fn \\ na, & nb \ldots nf & nn \end{vmatrix} \quad \ldots\ldots\ldots\ldots \quad \text{(iii)}$$

In his memoir on the orbit of *Pallas*, Gauss, for brevity, omitted the proof that aa, $(bb\,.\,1)$, $(cc\,.\,2)$ . . . are all positive, which is essential in order that the minimum value of Ω may be given by $A = 0$, $B' = 0$, &c. On p. 277 of the *Jahrbuch*, Encke gives a proof; but perhaps the most direct method of demon-

strating this elegant result is by exhibiting $(a\,a)$, $(a\,a)\;(b\,b\,.\,1)$, &c. as sums of squares: thus $(a\,a)$ is positive, and so is $(b\,b\,.\,1)$, for

$$(aa)(bb.1) = \begin{vmatrix} aa, & ab \\ ba, & bb \end{vmatrix} = \Sigma \begin{vmatrix} a_1, & b_1 \\ a_2, & b_2 \end{vmatrix}^2$$

Also

$$(aa)(bc.1) = \begin{vmatrix} aa, & ab \\ ca, & cb \end{vmatrix} = \Sigma \begin{vmatrix} a_1, & b_1 \\ a_2, & b_2 \end{vmatrix} \begin{vmatrix} a_1, & c_1 \\ a_2, & c_2 \end{vmatrix}$$

and

$$(bb.1)(cc.2) = \begin{vmatrix} bb.1, & bc.1 \\ bc.1, & cc.1 \end{vmatrix}$$

the constituents of this last determinant being of exactly the same form as in that to which $(a\,a)\;(b\,b\,.\,1)$ is equated; whence $(b\,b\,.\,1)\;(c\,c\,.\,2)$ is a sum of squares, and so on.

§ 8. The integral—

$$\int_{-\infty}^{\infty}\int_{-\infty}^{\infty} \ldots\; e^{-\Omega}\,dx\,dy \;\ldots\ldots\ldots\ldots \quad \text{(iv)}$$

evaluated by Gauss (*Theor. Mot.* § 182) and by Encke (*Jahrbuch*, p. 286) is when slightly transformed the same as that considered by Todhunter in his memoir "On the Method of Least Squares" (*Cambridge Phil. Trans.*, vol. xi.). The result obtained by Gauss and Encke for (iv) is—

$$\frac{\sqrt{\pi^6}}{\sqrt{\{(aa)(bb.1)(cc.2)(dd.3)(ee.4)(ff.5)\}}}\,e^{-(nn.6)} = \frac{\sqrt{\pi^6}}{\sqrt{\nabla}}\,e^{-(nn.6)}$$

∇ denoting, as before, the determinant whose principal diagonal is $a\,a$, $b\,b$, $c\,c$, $d\,d$, $e\,e$, ff, and the value of $n\,n\,.\,6$ being given in (iii). If we form from Ω a function Ω' by changing the sign of $n\,n$ and replacing the coefficients of the linear terms by the same quantities multiplied by $\sqrt{(-1)}$, viz. by replacing $(a\,n)$ by $a\,n\,\sqrt{(-1)}$ &c. and denote by Ω'' what Ω' becomes when the sign of $\sqrt{(-1)}$ is changed, then, u denoting the quadratic terms in Ω, we easily see that the integral considered by Todhunter (or rather that integral multiplied by the constant factor e^{-nn}), viz.—

$$\int_{-\infty}^{\infty}\int_{-\infty}^{\infty} \ldots\; e^{-u-nn}\cos(an.x+bn.y+\ldots)\,dx\,dy\ldots$$

$$= \frac{1}{2}\int_{-\infty}^{\infty}\int_{-\infty}^{\infty} \ldots\; (e^{-\Omega'} + e^{-\Omega''})\,dx\,dy\ldots$$

$$= \frac{\sqrt{\pi^6}}{\sqrt{\nabla}}\,e^{nn.6} \;\ldots\ldots\ldots\ldots\ldots \quad \text{(v)}$$

for the constituents that form the border of the numerator of $nn.6$ in (iii) each become multiplied by $\sqrt{(-1)}$ for Ω' and by $-\sqrt{(-1)}$ for Ω'', while nn is multiplied by -1, so that the only effect of the substitution of Ω' and Ω'' for Ω is to change the sign of $nn.6$. The value (iii) is identical with that given by Cayley at the end of Todhunter's paper, except for the presence of (nn), which is there (without any loss of generality) taken as zero. The mere evaluation of the integral when Ω contains only quadratic terms is, as remarked by Cayley, best effected at once by the theory of linear transformations (see Boole, *Cambridge Mathematical Journal*, vol. iii. p. 116); but, as a general method, Gauss's could scarcely be improved.

§ 9. In § 3 it was observed that in the case of three unknowns, x, y, z as obtained by the method of substitution required 23 tabular entries, while X, Y, Z were given by 12 more if Gauss's formulæ were used, and by 7 if Encke's method was followed. This naturally suggests an investigation of the number of tabular entries required in the different methods of obtaining the weights that are described in Encke's account in the general case when the number of unknowns is i, and such I proceed now to give. I should state that it is no more my opinion than it was Bessel's that the labour of a calculation is exactly proportional to the number of tabular entries required in its performance; but it will be generally admitted that the number of entries affords a good rough test when the methods to be compared are not unlike one another, and in the present case, the amount of work in each process is certainly measured in this way with a close approach to accuracy. The enumeration of the entries is also interesting on its own account.

Chauvenet calls all the quantities $aa, ab, \ldots\ bb.1, bc.1 \ldots$ &c., auxiliaries; but in what follows I restrict this name to mean merely those coefficients that have to be calculated in the course of the solution of the equations (i), the coefficients of which are supposed given originally; so that $aa, ab, ac \ldots$ are excluded, and only coefficients which contain a postfix are regarded as auxiliaries. It will be convenient to follow Encke and suppose there are only six unknowns, x, y, z, w, u, t—the leading astronomical case; but the reasoning in all cases will be perfectly general, and apply when the number of unknowns is i. It will be assumed, except where otherwise stated, that the fundamental order of the letters on which the meanings of the auxiliaries depend is that of the alphabet.

Consider, then, any auxiliary, say $ef.4$; it is defined by the equation

$$ef.4 = ef.3 - \frac{(ed.3)(fd.3)}{dd.3}$$

and is therefore dependent on the four auxiliaries $ef.3$, $ed.3$, $fd.3$, $dd.3$. In order, therefore, to calculate $ef.4$ we need the logarithms of three auxiliaries, viz., of $ed.3$, $fd.3$, $d.3$; and,
d

besides, one antilogarithm is required in finding the value of the second term of $ef.4$. As everything is expressed in terms of the auxiliaries, it is plain that (with the exception of the antilogarithms that give the actual values of the unknowns) no antilogarithm can be required, except in the calculation of the second term of an auxiliary, and that the calculation of every auxiliary must require one antilogarithm; thus, with the exception just noticed, the number of auxiliaries is equal to the number of antilogarithms required.

§ 10. The value of t, the sixth unknown, but which is first determined, is given (*Jahrbuch*, p. 271) by the equation

$$t = -\frac{fn.5}{ff.5}$$

Consider therefore the auxiliaries and coefficients on which $ff.5$ depends, and which have to be previously obtained: we easily see, from pp. 268–270 of the *Jahrbuch*, or independently, that these are

$$\begin{array}{llllll}
ff.5 \\
\\
ff.4, & fe.4 \\
 & ee.4 \\
\\
ff.3, & fe.3, & fd.3 \\
 & ee.3, & ed.3 \\
 & & dd.3 \\
\\
ff.2, & fe.2, & fd.2, & fc.2 \\
 & ee.2, & ed.2, & ec.2 \\
 & & dd.2, & dc.2 \\
 & & & cc.2 \\
\\
ff.1, & fe.1, & fd.1, & fc.1, & fb.1 \\
 & ee.1, & ed.1, & ec.1, & eb.1 \\
 & & dd.1, & dc.1, & db.1 \\
 & & & cc.1, & cb.1 \\
 & & & & bb.1 \\
\\
ff, & fe, & fd, & fc, & fb, & fa \\
 & ee, & ed, & ec, & eb, & ea \\
 & & dd, & dc, & db, & da \\
 & & & cc, & cb, & ca \\
 & & & & bb, & ba \\
 & & & & & aa
\end{array}$$

while for $f\,n.5$ are required, in addition,

$$\begin{array}{llllll} fn.5 \\ fn.4, & en.4 \\ fn.3, & en.3, & dn.3 \\ fn.2, & en.2, & dn.2, & cn.2 \\ fn.1, & en.1, & dn.1, & cn.1, & bn.1 \\ fn, & en, & dn, & cn, & bn, & an \end{array}$$

In the first scheme the number of auxiliaries that occur with any given postfix is always triangular, and we see that the number of antilogarithms or auxiliaries required for the calculation of the denominator of the first unknown that is determined is

$$= 1 + 3 + 6 + 10 \ldots + \frac{i.i-1}{2} = \frac{1}{2}\left\{\frac{i^3}{3} + \frac{i^2}{2} + \frac{i}{6} - \frac{i^2}{2} - \frac{i}{2}\right\} = \frac{1}{6}(i^3 - i);$$

the numerator requires $\frac{1}{2}(i^2 - i)$ additional antilogarithms, so that the number of antilogarithms required in order to find the logarithm of the first unknown determined $= \frac{1}{6}(i^3 + 3i^2 - 4i)$ $= \frac{1}{6}\, i.i - 1.i + 4$, agreeing with the formula given by Encke for the number of auxiliaries (*Jahrbuch*, p. 272).

Before proceeding further, it will avoid circumlocution to denote the i unknowns in the general proposition by $x_1, x_2 \ldots x_i$, so that by the method of substitution we first determine x_i from the equation

$$x_i = -\frac{\mu n.i-1}{\mu\mu.i-1}$$

μ being the i^{th} letter of the alphabet, and then determine x_{i-1} from x_i and so on. Thus we shall first count the number of entries requisite to determine $\log x_i$, then one antilogarithm will give x_i, and we shall proceed to examine afterwards how many more entries are required in finding $x_{i-1}, \ldots x_1$. Sometimes the special letters and numbers having reference to the case of $i=6$ will be used, and sometimes the general symbols just mentioned, but no confusion can ever arise from this cause.

Having enumerated the antilogarithms requisite for the determination of $\log x_i$, it remains to find the number of logarithms. A little consideration shows that in the schemes written above, we require the logarithm of every auxiliary or coefficient that forms the extreme right-hand constituent of a line, and of no other constituent. The number of logarithms amounts therefore to

$$(1 + 2 + 3 \ldots \quad + i) + i$$

(the last term being due to the auxiliaries that involve n)

$$= \frac{1}{2}(i^2 + 3i).$$

The total number of entries, therefore, that are required in finding $\log x_i$

$$= \frac{1}{6}(i^3 + 6i^2 + 5i)$$

of which $\frac{1}{2}(i^2+3i)$ are for logarithms, and $\frac{1}{6}(i^3+3i^2-4i)$ for antilogarithms.

An examination of the *Endgleichungen* (*Jahrbuch*, p. 272), shows that for the calculation of x_{i-1}, x_{i-2}, ... x_1 we require $i-2$ logarithms and $\frac{1}{2}(i^2+i)$ antilogarithms: so that the solution of the equations and determination of x_1, x_2, ... x_i, require $\frac{1}{6}(i^3+9i^2+14i-12)$ tabular entries; of which $\frac{1}{2}(i^2+5i-4)$ are for logarithms, and $\frac{1}{6}(i^3+6i^2-i)$ for antilogarithms. It is proper to remark, that this number of entries gives us x_1, x_2, ... x_i, and also $\log x_2$, ... $\log x_i$, which are required in obtaining x_1, x_2, ... x_i, but not $\log x_1$, so that if the logarithms of x_1, x_2, ... x_i are wanted, as well as their simple values, only one fresh entry is required. From the value of $nn. 6$, viz. $nn - \frac{(an)^2}{aa} - \frac{(bn.1)^2}{bb.1} - \frac{(cn.2)^2}{cc.2} - \frac{(dn.3)^2}{dd.3} - \frac{(en.4)^2}{ee.4} - \frac{(fn.5)^2}{ff.5}$ (*Jahrbuch*, p. 279), we see that the calculation of (vv) only involves i additional antilogarithms, so that x_1, x_2, ... x_i and (vv) require $\frac{1}{6}(i^3 + 9i^2 + 20i - 12)$ entries; $\log(vv)$ would require another entry.

§ 11. I now proceed to the different methods of obtaining the weights, between which the comparison really lies, as the mode of solution of the equations and calculation of (vv) is common to all the methods. The three modes of practically determining the weights which will now be considered are Gauss's (*Jahrbuch*, pp. 308, 309, and *Theoria Comb. Obs.*, § 32), and the two which are explained on pp. 313 and 314 of the *Jahrbuch*, and which, as I do not think they are given by Gauss, are probably due to Encke himself. To begin with Gauss's method, it will be seen on reference to the equations on pp. 308 and 309 of the *Jahrbuch* that, separating the numbers of logarithms and antilogarithms by a comma, $\log A'$, requires 0, 0; $\log A''$, 1, 2; $\log A'''$, 1, 3; $\log A^{iv}$, 1, 4; $\log A^{v}$, 1, 5; $\log B''$, 0, 0; $\log B'''$, 1, 2; $\log B^{iv}$, 1, 3; $\log B^{v}$, 1, 4; $\log C'''$, 0, 0; $\log C^{iv}$, 1, 2; $\log C^{v}$, 1, 3; $\log D^{iv}$, 0, 0; $\log D^{v}$, 1, 2; $\log E^{v}$, 0, 0. Thus the A's require 3+4+5

$+6$ entries, the B's, $3+4+5$, the C's $3+4$, and the D's 3; and generally the logarithms of the factors require

$$3 + (3 + 4) \ldots + (3 + 4 \ldots + i)$$

$$= \Sigma^{*}\frac{1}{2}(i + 3 . i - 2) + 2$$

$$= \frac{1}{2}\left\{\frac{i^3}{3} + \frac{i^2}{2} + \frac{i}{6} + \frac{i^2}{2} + \frac{i}{2} - 6i\right\} + 2$$

$$= \frac{1}{6}(i^3 + 3i^2 - 16i + 12)$$

entries. The deduction of the logarithms of the weights from the factors requires

$$3 + 4 \ldots + (i + 1) = \frac{1}{2}(i^2 + 3i - 4)$$

entries, so that altogether the determination of the logarithms of the weights by Gauss's method requires

$$\frac{1}{6}(i^3 + 6i^2 - 7i) = \frac{1}{6}\, i.i - 1.i + 7$$

entries, of which $\frac{1}{2}(i^2 - i)$ are for logarithms and $\frac{1}{6}(i^3 + 3i^2 - 4i)$ for antilogarithms. We may well close the enumeration with the determination of the logarithms of the weights, as the weights themselves are not essential. Their calculation requires $i - 1$ additional entries (all for antilogarithms), in both Gauss's and Encke's first method, and $i - 2$ additional entries (all for antilogarithms), in Encke's second method: log (vv) requires an additional logarithm in all three methods.

In Encke's first method the weights are determined from the following equations:—

$$ff.5 = ff.5$$

$$ee.5 = (ee.4)\frac{ff.5}{ff.4}$$

$$dd.5 = (dd.3)\frac{ee.4}{ee.3}\cdot\frac{ff.5}{(ff.4)_d}$$

$$cc.5 = (cc.2)\frac{dd.3}{dd.2}\cdot\frac{ee.4}{(ee.3)_c}\cdot\frac{ff.5}{(ff.4)_c}$$

$$bb.5 = (bb.1)\frac{cc.2}{cc.1}\cdot\frac{dd.3}{(dd.2)_b}\cdot\frac{ee.4}{(ee.3)_b}\cdot\frac{ff.5}{(ff.4)_b}$$

$$aa.5 = (aa\ \ )\frac{bb.1}{bb}\cdot\frac{cc.2}{(cc.1)_a}\cdot\frac{dd.3}{(dd.2)_a}\cdot\frac{ee.4}{(ee.3)_a}\cdot\frac{ff.5}{(ff.4)_a}$$

* Σ here and in the rest of the paper means summation with regard to i form $i = 1$ to $i = i$.

wherein the literal suffixes indicate that the fundamental order of the letters is supposed to be changed, and point out which letter is removed from its original place and put last; thus $(ee.3)_c$ means $ee.3$ when the order of the letters is a, b, d, e, f, c. Whenever no literal suffix is added, the order is the fundamental order a, b, c, d, e, f. We notice that when any letter is transferred from its place to the end, the significations of all those auxiliaries that have a postfix greater than that corresponding to the letter shifted are altered; thus when c is put last, no change is made in the auxiliaries with postfixes 1 and 2, but all having higher postfixes receive different meanings. Thus, in counting the new auxiliaries we leave off, in the schemes written below, just before the line containing those postfixes that correspond to the letter which has been placed last.

Construct the following schemes:—

(e last)

$$ff.4$$

(d last)

$$\begin{array}{ll} & ff.4 \\ ee.3; & ff.3,\ fe.3 \end{array}$$

(c last)

$$\begin{array}{lll} & & ff.4, \\ & ee.3 & ff.3,\ fe.3, \\ dd.2; & ee.2,\ ed.2; & ff.2,\ fe.2,\ fd.2 \end{array}$$

(b last)

$$\begin{array}{lll} & & ee.3, \\ & dd.2, & ee.2,\ ed.2, \\ cc.1; & dd.1,\ dc.1; & ee.1,\ ed.1,\ ec.1 \end{array}$$

$$\begin{array}{l} ff.4, \\ ff.3,\ fe.3, \\ ff.2,\ fe.2,\ fd.2, \\ ff.1,\ fe.1,\ fd.1,\ fc.1 \end{array}$$

in which the law of formation is sufficiently evident to render it unnecessary to write down the scheme for a last. It will be seen that these triangular groups (omitting the bottom line of each) include all the new auxiliaries that are required; by "new auxiliaries" being meant auxiliaries which have a different meaning from what they had originally when the order was a, b, c, d, e, f, and which in the formulæ for the weights have literal suffixes. For suppose c put last so that the order is a, b, d, e, f, c, then to find what auxiliaries $fe.3$ (say) immediately depends upon, we diminish the postfix by 1, and so obtain $fe.2$, and then for the second term we have $fd.2$, and $ed.2$, besides

the denominator $dd.2$, d being the letter corresponding to the postfix 2; we therefore retain $fe.2$ and $fd.2$, but $ed.2$ has been already counted in the scheme of which $ee.3$, is the final auxiliary; and $dd.2$ has also been counted. Thus taking the letters (and postfixes) always in order, we see that in counting the number of new auxiliaries required for $ff.4$, that have not previously been counted, we retain only those that involve an f as one of their letters, and any other auxiliary that arises must also have arisen previously. The reason for the schemes is thus apparent, and we remark that the bottom line of each triangular group consists of auxiliaries that were previously required in the process of solution for $x, y, z \ldots$, so that the number of new auxiliaries, which is equal to the number of new antilogarithms,

$$= 1 + (1+3) + (1+3+6)\ldots + \left(1+3+6\ldots+\frac{i-2.i-1}{2}\right)$$

$$= 1 + (1+3)\ldots + \frac{1}{6}(i^3-3i^2+2i) = \frac{1}{6}\Sigma(i^3-3i^2+2i)$$

$$= \frac{1}{24}(i^4-2i^3-i^2+2i) = \frac{1}{24}\,i.i-1.i-2.i+1$$

With regard to the number of logarithms, a little consideration shows that only the logarithms of the extreme right-hand auxiliaries in all the horizontal rows (including the bottom rows) of the triangular groups are required; so that the number of logarithms

$$= 1 + (1+2) + (1+2+3)\ldots + (1+2\ldots+i-1)$$

$$= \frac{1}{2}\Sigma(i^2-i) = \frac{1}{6}(i^3-i) = \frac{1}{6}i.i-1.i+1$$

and the whole number of additional entries required in the calculation of the logarithms of the weights

$$= \frac{1}{24}(i^4-2i^3-i^2+2i) + \frac{1}{6}(i^3-i)$$

$$= \frac{1}{24}(i^4-2i^3-i^2-2i) = \frac{1}{24}\,i.i-1.i+1.i+2 \quad \ldots\ldots\ldots \quad \text{(vi)}$$

The above is here called Encke's first method; but in reality he scarcely proposes it as a separate process that ought to be pursued, but only regards it as subsidiary to the method (here called his second) which he recommends, and to which I now proceed. It is, however, convenient to regard his investigation as two methods, as the one here called the first is that which was found so successful in the case of three unknowns (*see* § 3).

In Encke's second method the first three weights $ff.5$, $ee.5$, $dd.5$, are to be calculated as just described in the first method;

but the other three are to be obtained by beginning the whole process *de novo*, and solving the equations afresh with reversed order of letters, viz., f, e, d, c, b, a. We have thus a complete verification of the values found for x, y, z ...; and the last three weights are obtained exactly as the first three were.

First, considering only the solution of the equations, it appears that only one entry, viz., that for the logarithm of fa is common to the two processes (so that the verification is very complete); thus the re-determination of the values of the unknowns and of $(v\,v)$ requires

$$\frac{1}{6}\,(i^3+9\,i^2+20\,i-18)$$

entries. Then if i be even, it will be easily seen that the number of entries required in the determination of the weights is found, by writing $\frac{1}{2}\,i$ for i in (vi) and doubling the result, so that the number of entries

$$=\frac{1}{12}\left(\frac{i^4}{16}+\frac{i^3}{4}-\frac{i^2}{4}-i\right)$$

If i be uneven the number of entries is found by substituting $\frac{1}{2}\,(i-1)$ and $\frac{1}{2}\,(i+1)$ for i in (vi) and adding the results, we find the number

$$=\frac{1}{192}\,(i^4+4\,i^3+2\,i^2-4\,i-3)$$

A very little consideration is required in order to satisfy oneself that in the determination of the logarithms of the weights, none of the entries are common to the formulæ in which a and f are respectively the first letters; perhaps this is best seen by fixing the attention on the denominators of the auxiliaries (*see* § 6). Thus any auxiliary with postfix 4 (say) has for its denominator the determinant whose principal diagonal is $\alpha\,\alpha$, $\beta\,\beta$, $\gamma\,\gamma$, $\delta\,\delta$; α, β, γ, δ being the first four letters in the assumed order; so that no auxiliary in which the order is a, b, c, d, e, f can coincide with one in which it is f, e, d, c, b, a.

The difficulty of the enumeration consists rather in seeing that no entries are counted twice, than that none are passed over. I have given the explanation of the processes of the enumeration in sufficient detail to make them easy to follow, so that in case an error should have been made, its correction may be easy.

The general results that have been obtained are, (1.) that, i being the number of unknowns x_1, x_2, . . . x_i, the solution of the equations and the determination of $(v\,v)$ (but not of log $v\,v$) require

$$\frac{1}{6}\,(i^3+9\,i^2+20\,i-12)$$

entries; and that we have also incidentally log x_2, log x_3 . . ,

$\log x_i$, but not $\log x_1$; (2.) that Gauss's mode of determining the logarithms of the weights requires

$$\frac{1}{6}(i^3+6i^2-7i) \quad \dots\dots\dots \text{(vii)}$$

entries; (3.) that Encke's first method of determining the same requires

$$\frac{1}{24}(i^4+2i^3-i^2-2i) \quad \dots\dots\dots \text{(viii)}$$

entries; and (4.) that his second method requires

$$\frac{1}{6}(i^3+9i^2+20i-18)+\frac{1}{12}\left(\frac{i^4}{16}+\frac{i^3}{4}-\frac{i^2}{4}-i\right)$$

or $\dots\dots$ (ix)

$$\frac{1}{6}(i^3+9i^2+20i-18)+\frac{1}{192}(i^4+4i^3+2i^2-4i-3)$$

entries, according as i is even or uneven; but that this last method gives us also a re-calculation of $x_1, x_2, \dots x_i$ and $(v\,v)$. If the weights be included as well as their logarithms, $i-1$ must be added to (vii) and (viii) and $i-2$ to (ix). From the formulæ (vii), (viii), (ix), the following Table was constructed:

i	Number of entries required in determining				Total number of entries		
	$x_1, x_2, \dots x_i$ and (vv),	the logarithms of the weights,					
		by Gauss.	by Encke, I.	by Encke, II.	by Gauss.	by Encke, I.	by Encke, II.
3	26	10	5	26	36	31	52
4	46	22	15	47	68	61	93
5	73	40	35	78	113	108	151
6	108	65	70	117	173	178	225
7	152	98	126	171	250	278	323
8	206	140	210	235	346	416	441
9	271	192	330	320	463	601	591
10	348	255	495	417	603	843	765
11	438	330	715	542	768	1153	980
12	542	418	1001	681	960	1543	1223

It will be thus seen, that though Encke's first method requires less tabular entries for $i=3, 4, 5$, yet Gauss's is far preferable for higher values of i, and this advantage of course increases rapidly as his formula involves i^3, while Encke's has a term in i^4. Encke's second method has the great merit of affording a complete verification for everything except the determination of the

logarithms of the weights. It is only fair to notice expressly that Encke's remarks are no doubt only intended to apply to the case of $i =$, or less than, 6; and most likely he did not at all consider the relative advantages of the methods when i exceeded this limit, as such cases were scarcely likely to occur often in astronomical or other applications. If i were large, it might be worth while to extend Encke's second method and take the letters in a third order, so as to obtain a triple determination of $x_1, x_2, \ldots x_i$ and $(v\,v)$, to serve as a verification, not of the values of the unknowns, but of some of the fresh auxiliaries. Where the methods are as similar as those which have just been compared, the number of tabular entries appears to afford a very good test of the amount of work required in the performance of each.

§ 12. As an immediate corollary to the enumeration at the beginning of § 10, I add the number of entries required in the calculation, by the method of substitution, of a symmetrical determinant of i rows:

$$\begin{vmatrix} a\,a, & a\,b, & a\,c, & \ldots \\ b\,a, & b\,b, & b\,c, & \ldots \\ c\,a, & c\,b, & c\,c, & \ldots \\ \ldots & \ldots & \ldots & \ldots \end{vmatrix}$$

which $= (a\,a)\,(b\,b\,.\,1)\,(c\,c\,.\,2)\ldots$

An examination of the schemes in § 10 readily shows tha the number of antilogarithms required is

$$1 + 3 + 6 \ldots + \frac{i\,.\,i-1}{2} = \frac{1}{6}\,(i^3 - i)$$

and the number of logarithms $= \frac{1}{2}\,(\,i^2 + i\,)$; to these must be added the final antilogarithm, so that the total number of tabular entries required is

$$\frac{1}{6}\,(i^3 + 3\,i^2 + 2\,i + 6) = \frac{1}{6}\,(i+3)\,(i^2+2)$$

It is very likely that the method of substitution affords the most simple mode of calculating a symmetrical determinant, and that the number of entries could not be conveniently reduced below that just found.*

§ 13. In what precedes the equations (i) to be solved have been supposed to be given, and we have been in no way con-

* I asked Professor Cayley if he knew an expression for the number of terms in the development of a symmetrical determinant, in order that I might compare the number of entries required in the above procedure with what would be necessary if the expanded determinant were calculated directly, and he in consequence investigated the very elegant formula contained in the paper published in the April number, pp. 303–307. The number of terms in the expanded determinants of the third, fourth, fifth, and sixth orders being respectively, as found by Professor Cayley, 5, 17, 73, 388, it follows that the number of entries required would be 11, 27, 88, 409, while the substitution method requires only 11, 21, 36, 57.

cerned with the manner in which they were obtained; but I cannot close this paper without alluding to the very elegant method proposed by Bessel for the formation of the coefficients $(a\,a)$, $(a\,b)$,... by a process not unlike that of multiplication by quarter-squares. In No. 399 of the *Astr. Nach.* (vol. xvii. May 7, 1840), Bessel points out how, if there are μ equations of condition and i unknowns (and if the coefficients and not their logarithms be given), by the use of a table of squares, instead of a table of logarithms, $\mu\ (i + 1)$ tabular entries may be saved; for, calculate by the aid of the table of squares $(a\,a)$, $(b\,b)$, $(c\,c)$... then, say, $(a\,b) = \frac{1}{2}\left\{(a + b . a + b) - (a\,a) - (b\,b)\right\}$ and is given by the μ entries required for $(a + b . a + b)$, the same number as $(a\,b)$ would require if $\log a$, $\log b$.... were known; but $(a\,a)$, $(b\,b)$... require only $\mu\ (i + 1)$ entries, the same as would be requisite for $\log a$, $\log b$... so that on the whole the $\mu\ (i + 1)$ (antilogarithmic) entries that the calculation of $(a\,a)$, $(b\,b)$... by logarithms would require are saved. A calculation performed by n-figure logarithms is generally (nearly) correct to n figures, the only way in which figures get lost being when two terms, in both of which the first figure is of the same real and local value, have to be subtracted the one from the other, when the result is true to one figure less than either. This is often the cause of great inconvenience in logarithmic calculations, but Bessel's method by squares is liable to a much greater drawback, for in obtaining $a\,b$ from the formula $\frac{1}{2}\left\{(a + b)^2 - a^2 - b^2\right\}$ if a be large and b be small (so that the product is small), we obtain scarcely any more accuracy than if b had been large too, as we lose figures by the subtraction. Bessel noticed this himself, as he lays stress on the fact that the coefficients should not differ much among themselves, and points out how the units may be altered so as to make them more equal; but a good deal of the advantage of the method is lost if the equations have to be previously prepared. The great merit that multiplication by means of quarter squares possesses over multiplication by logarithms is, that the results are accurate, and not approximate; but here, where the table of squares would only extend to a certain number of figures, this advantage is lost. Bessel used only a small table giving 2,000 squares; but a fairly large table for the same purpose is added by Faà de Bruno at the end of his *Calcul des Erreurs.* The products of every pair of coefficients in all the equations of condition have to be found, so that one summation-product contains as a term the product of the greatest and least coefficients; and here the loss of figures might be serious: and what is worse, this loss would be concealed in the method of calculation, so that it would only be apparent when sought out, and the term in question examined separately. But theoretically the method is very pretty, and Bessel speaks highly of it as the result of practical experience.

Addition to the Paper on the Number of distinct Terms in a Symmetrical, or partially Symmetrical Determinant. By Professor Cayley.

The generating function

$$u, = 1 + u_1 x \ldots \quad + u_n \frac{x^n}{1.2\ldots n} + \ldots, = \frac{e^{\frac{1}{2}x+\frac{1}{4}x^2}}{\sqrt{1-x}}$$

was obtained as the solution of the differential equation

$$2\frac{du}{dx} = u\left(1+x+\frac{1}{1-x}\right).$$

Writing this in the form

$$2\,(1-x)\frac{du}{dx} = u\,(2-x^2),$$

we at once obtain from u_n the equation of differences,

$$u_n = n u_{n-1} - \tfrac{1}{2}(n-1)(n-2)\,u_{n-3};$$

and it thus appears that the values of u_n (number of distinct terms in a symmetrical determination of the order n) can be calculated the one from the other by the process

$n = 1,$	$1 = 1.1$
$= 2,$	$2 = 2.1$
$= 3,$	$5 = 3.2 - 1.1$
$= 4,$	$17 = 4.5 - 3.1$
$= 5,$	$73 = 5.17 - 6.2$
$= 6,$	$388 = 6.73 - 10.5$
	&c.

which is one of extreme facility.

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*Continued Observations of the Companion of Procyon.** By Herr Struve.

(*Translation.*)

The months of February and March were this year singularly unfavourable for Astronomical observations. In the few hours which were free from clouds, the images were for the most part so tremulous, that micrometrical measurements even of objects of comparatively little difficulty could scarcely be undertaken. Not until the 21st of March did I succeed in obtaining

* The substance of this paper was delivered orally by M. Struve, at the evening meeting, on May 8. The paper, which is written in German, has been kindly translated by W. T. Lynn, Esq., B.A.—[Ed.]

a tolerably satisfactory measure of the companion of *Sirius*. Although on that evening the images were still not of the best, yet I could not neglect the opportunity of at once examining *Procyon* in reference to the small point of light which I had remarked in its neighbourhood last year. On looking into the telescope accordingly on that night, I thought that I recognised by glimpses such an object with some certainty, of which I estimated the position-angle at about 95°; and my assistant, Herr Lindemann, succeeded in making a determination agreeing within 0°·1 with my approximate one, unknown to him when making his. Immediately after this the images became so tremulous again, that the point of light could no longer be perceived. On the 1st of April the images were better, but streaks of nebulous cloud made continuous observation impossible. During some short intervals free from cloud, we succeeded in making only three separate hasty measures of the position-angle, the mean of which gives 93°·3: a determination to which, on account of the haste with which it was necessarily made, I should only attribute a small degree of accuracy.

The following week the sky was again constantly clouded. Not till the 9th of April could I obtain the first complete measurement; after which three others, on April 13 to 15, were made, under very good atmospherical circumstances. The observations were all made with the Power III. of 309 times. Each position-angle below depends upon three determinations for the distances; the number of measures is affixed to each in parentheses.

Date.		Time of Obs.	Distance and	No. of Measures.	Position of Instrument.	Time of Obs.	Position-Angle.	Position of Instrument.
		h m	″			h m	°	
1874	April 9	9 40	10·47	(2)	I.	9 5	97·0	I.
						9 30	94·8	I.
	April 13	9 35	12·17	(3)	I.	9 28	96·6	I.
						9 40	94·5	I.
						9 54	98·0	II.
	April 14	9 50	12·04	(2)	II.	9 38	94·9	II.
						10 1	99·4	II.
	April 15	9 40	12·00	(3)	I.	9 33	92·8	I.
						9 46	97·9	I.

Besides these, Herr Lindemann made the following measures of the angle of position:—

April	9	99·3°
,,	13	94·8
,,	15	96·6
Mean		96·9

In regard to the measures of distance, the agreement on the three last evenings has been remarkably favoured by fortuitous circumstances, for the separate determination on those evenings differ from each other by a whole second and more, as might indeed be expected from the difficulty of the object. It is not, therefore, surprising that the distance on the 9th of April should be smaller by $1''\cdot70$. Last year the differences on separate evenings were considerably greater.

To my observations the systematic corrections derived from measures with artificial double stars must be applied. These are for the distances quite insensible, since they amount in the case before us to only $0''\cdot01$ at the maximum. For the position-angles, on the other hand, they are considerable. Subject to small variations for the different hour-angles, they amount at the mean to $2^\circ\cdot95$.

I would here remark, that in my paper of last year on the amount of the systematic correction, an error has been overlooked. It amounts for last year in the mean for Power III. to $+ 2^\circ\cdot66$ and for Power IV. to $1^\circ\cdot61$.

Now, if we take the mean values from my observations for the two years, we have

1873	March 28	$d = 12''\cdot49$	$P = 87^\circ\cdot65$
1874	April 10	11·67	96·65

or, after applying the systematic corrections:

1873	March 28	$d = 12''\cdot49$	$P = 90^\circ\cdot24$
1874	April 10	11·67	99·60

The distance in the interval would, therefore, seem to have diminished by about $0''\cdot8$. But owing to the difficulty of the measures, the mean values themselves must be subject to such uncertainties that the reality of the apparent diminution must be considered doubtful. But in regard to the increase of the position-angle, there can scarcely be any doubt. Not only does the observed increase of $9^\circ\cdot5$ correspond to a considerable linear change of place, amounting to $2''\cdot0$, but the measures of direction are in themselves much easier, and more certain than those of distance.

It is well known that Professor Auwers, as soon as he had received my observations of last year, repeated his investigations into the variable proper motion of *Procyon*, availing himself also of the observations of this star which has been made since 1862. From this he concluded that it was doubtful whether the object observed by me was really the sole body disturbing the proper motion of *Procyon*, but that the doubt would be removed if it appeared this spring that the position-angle had undergone an increase of from 9° to 10°. And this increase has really shown itself above in the most remarkable manner. I consider it,

therefore, to be decisively established that the object I have observed is actually the companion whose existence has been theoretically proved by the calculations of Auwers; and hope that the astronomical world will rejoice with me in the triumph thus obtained for the labours of my honoured friend, and through them for our common science. In order to remove any exception that might be taken that the wished-for result had in any degree been itself the cause of the recognition, and affected the measurement of the place of so difficult an object, I will just remark, that I had not looked again at the paper of Auwers in question since its first receipt last summer, and had totally forgotten the data of its criterion, and the mutual relation of the two stars. I did not again take it up until after I had succeeded in making the first observation, and the results of that paper were even less present to the mind of my assistant, Herr Lindemann, whose younger eye appears generally to have seen the companion even better than mine.

On the other hand, we cannot lose sight of the important fact that various other astronomers, using very powerful instruments last winter and spring, have, so far as has hitherto been known to me, sought in vain for the faint companion. In particular, this has been the case with the new refractor of the Washington Observatory of 26 inches aperture, with the performance of which Professor Newcomb expresses himself as well satisfied. Of the cause of the failure I can, not being in possession of detailed information concerning the circumstances of the observations, merely form conjectures. Perhaps the atmospheric conditions during the Washington observations were less favourable than those here; perhaps also the surpassing brilliancy of the principal star may have hindered the recognition of the small companion in its neighbourhood, if the observer made use of the whole aperture of 26 inches. How great is the effect of the brilliancy of the principal star has shown itself very clearly in our observations. Although the near companion is doubtless considerably brighter than the known little star, which is near *Procyon* without belonging to its system, and precedes it about 42″ to the north, the latter could always be seen in a dark night with ease by all observers who could not see the former at all, which was best seen in feeble twilight, whilst the more distant little star was not visible at all. But perhaps, also, practice in the recognition of such objects, and the greater or less sensibility of the retina, may have had their influence. In this respect also our observations have furnished some remarkable facts. In the last favourable observing evenings I requested six other astronomers to look successively at the interesting object. But whilst Herr Lindemann and myself saw and measured it with comparative ease; of the six in question only one—Herr Ceraski, second Assistant at the Moscow Observatory—succeeded in seeing it with certainty without being warned of the place where it was to be looked for. Of the others some thought they perceived it after

its place had been accurately pointed out to them, but not with satisfactory certainty. I myself always made the observations best if I came to the telescope with fresh eye. After some minutes the eye became usually so exhausted that I was obliged to give it some rest to enable me to see the object. Some of the later observations on separate evenings are therefore of inferior accuracy.

The difficulty of the observation of the companion of *Procyon* will of course be unequally greater than that of the companion of *Sirius*. In our latitudes this difficulty is somewhat equalised by the low position of *Sirius*: for the companion of which I have looked in vain on many evenings in the years immediately after its discovery by Alvan Clark, until at last favourable atmospheric conditions enabled me to see it distinctly. But after I had once recognised it, and thereby followed the modifications of its appearance with tremulous images, it is now only when there is a high degree of tremor that I cannot see it, at least by glimpses, with certainty. These experiences have doubtless been of use to me in the recognition of the companion of *Procyon*, and give me also full confidence in the accuracy of my perceptions, independently of the corroborating observations of Lindemann and Ceraski. Notwithstanding this, however, I willingly acknowledge that a confirmation of them by other observers at other places with other instruments, would be very welcome to me.

On a Method of finding the Parallax of Double Stars, and on the Displacement of the Lines in the Spectrum of a Planet.

By Professor C. Niven.

(*Communicated by W. H. M. Christie, Esq.*)

§ 1. The idea of Doppler that the motion of the fixed stars towards or from the Earth must exercise an influence on the nature of the light which we receive from them, though it has not served his purpose of explaining their colours, has yet not been without fruit. For although, in general, no great change of colour can be looked for in this direction, we may nevertheless expect to find traces of the effect in a displacement of the fixed lines of the spectrum. The displacement of the line F of hydrogen for a velocity of 20 miles per second amounts to nearly $\frac{1}{12}$th of the distance between the constituents of the double line of sodium, and is towards the violet or red end, according as the star is approaching or receding from us. Now displacements of this magnitude are fully within the reach of modern instruments, and accordingly Mr. Huggins has succeeded in estimating the velocities of separation or approach of a number of the brighter stars.

At the meeting of the British Association in Edinburgh, Mr. Fox Talbot suggested the application of the spectroscopic method to the determination of the distances of certain double stars. It is manifest that if we know the form of the orbit of a binary system, and its position with regard to the plane of the heavens, or in other words, of its apparent orbit, we require only one linear element to determine its absolute magnitude. And if this be given, the parallax of the system can be readily found; and, moreover, if we suppose, as there is every reason to do, that the law of gravitation holds good for such star systems, we have the means of determining the sum of their masses.

The linear element required would be supplied by an observation of the component in any direction of the relative velocity of the two members of the system, as, for instance, the component along the line joining the Earth with the star. For this depends on the orbital motion only, and not on any proper motion which the system as a whole may possess.

The difficulties in the way of effecting this determination arise from three sources:—the faintness of the stars, the smallness of their angular separation, and possibly also from the component of velocity under consideration being too small to be capable of measurement. With regard to the first two of these, it may be observed that Mr. Huggins has recorded in the *Philosophical Transactions* for 1864, that he was able to examine separately the two components of *β Cygni*, and of *α Herculis*. Now the magnitudes of the two components of the former of these are respectively 3 and 7 (Smyth), while those of the latter are $3\frac{1}{2}$ and $5\frac{1}{2}$, and are separated by an angle of $4''\cdot6$. It is therefore inferior in both respects to *Castor*, the two members of which are of magnitudes 3 and $3\frac{1}{2}$, and which were separated at the beginning of the year 1869 by a distance of nearly $6''$.

With regard to the third difficulty, it is only in one case, that of *α Centauri*, that we can speak with any certainty. Adopting the parallax of $0''\cdot913$, and the old elements calculated by Captain Jacob, I find the greatest velocity of approach to amount to 12·556 miles, and the greatest velocity of separation to be about 25·85 miles per second. With the elements more recently given by Mr. Powell, the greatest velocities of approach and separation are respectively only 6·82 and 20·41 miles; while about the present epoch the velocity is one of separation amounting to about 17·43 miles per second.

Under these circumstances there seems good ground for believing that the observations required are not beyond the reach of the present instrumental means; I have therefore thought that some service might be done by removing the geometrical obscurities of the problem, and showing what the formulæ are which serve, by the aid of the observations, to determine the parallax.

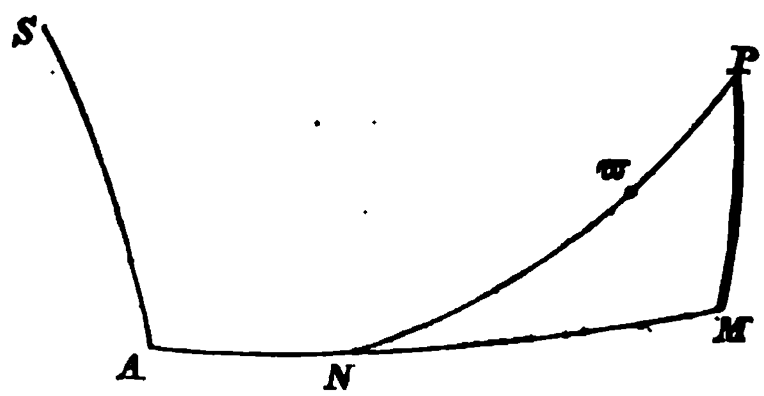
The general result of the investigation is a relation of the

form $p\,W = A \cos\theta + B$; wherein θ is the position of the secondary measured on the relative orbit from the node, p the parallax of the system (in seconds), W the component of the relative velocity measured *from* the Earth along the line to the system, and A and B two constants to be calculated in each case from the elements of the orbit. Here, as throughout the present paper, W is taken to be positive when considered as a velocity of separation, and negative when one of approach.

Some general deductions, which can be made from the above formulæ, will be considered in their proper place.

In Articles 5 and 6 I have considered the effect which the motion of a planet has on the lines in the spectrum of the light reflected from it.

§ 2. In considering the motion of a binary star system we take for plane of reference the tangent plane to the heavens, for the initial line that in which this plane is cut by a declination circle, and the direction of measurement to be north—following—south—preceding. It seems sufficiently established that the relative motion of the system may be represented by supposing the secondary component to trace out an ellipse in a plane inclined to the plane of reference conformably with the ordinary laws of elliptic motion, and having the primary as focus.



Let S be the primary, A the origin of measurement, N the node of the orbit, ϖ periastre, and P any position of the secondary; let, also, the arc P M be drawn at right angles to the plane of reference. Denote as usual the elements of the actual ellipse by a, e, N, i, ϖ, and by T the time of revolution in years. Let N P $= \theta$, N M $= \theta$, and P M $= \lambda$.

We thus have, first of all,

$$\left.\begin{aligned} \text{angle of position} &= N + \theta_1 \\ \tan\theta &= \tan\theta_1 \sec i \\ \sin\lambda &= \sin i \sin\theta \end{aligned}\right\} \quad \ldots\ldots\ldots \quad (1)$$

To connect the actual and angular mean distances a and a'', we have

$$a : \nabla :: \alpha : p \quad \ldots\ldots\ldots\ldots \quad (2)$$

∇ being the diameter of the Earth's orbit.

If we call S P $= r$, we find from the theory of elliptic motion:

$$\frac{1}{r} = \frac{\mu}{h^2}\left(1 + e\cos\overline{\theta - \varpi}\right)$$

$$\frac{dr}{dt} = \frac{\mu e}{h}\sin\overline{\theta - \varpi}$$

$$r\frac{d\theta}{dt} = \frac{\mu}{h}\left(1 + e\cos\overline{\theta - \varpi}\right).$$

The motion of the star is compounded of the velocities $\frac{dr}{dt}$ along S P and $r\frac{d\theta}{dt}$ at right angles to it. The component of the former at right angles to the plane of the apparent orbit is

$$\frac{dr}{dt}\sin\lambda, = \frac{\mu e}{h}\sin i \sin\theta \sin\overline{\theta-\varpi},$$

and that of the latter is

$$r\frac{d\theta}{dt}\sin i\cos\theta.$$

Thus, the whole component of velocity, W

$$= +\frac{\mu}{h}\sin i\,(\cos\theta + e\cos\varpi).$$

Now we have also

$$\frac{\mu}{h} = \frac{2\pi a}{\mathrm{T}\sqrt{1-e^2}},$$

whence, if W be measured in miles per second, and n be the number of seconds in a year,

$$\mathrm{W}.\mathrm{T}.n = \frac{2\pi a\sin i.\nabla}{p\sqrt{1-e^2}}(\cos\theta + e\cos\varpi) \quad . \quad . \quad . \quad . \quad (3)$$

[It is assumed in this result that the motion of the secondary in its apparent orbit is *direct*.]

§ 3. It follows from the expression just found that W attains its greatest values at the two nodes, being a positive maximum at the one and a negative maximum at the other. Of the two that is the greater which corresponds to the node nearest periastre, and this will be positive or negative according as the star in passing through periastre recedes from or approaches the nearest node. At two points situated symmetrically with respect to the line of nodes W vanishes. These points separate the region of the orbit from which W is positive from that in which it is negative, and are determined by $\theta = \pm\beta$, where $\cos\beta = -e\cos\omega$.

If we write p W $= \mathrm{A}\cos\theta + \mathrm{B}$, the two maxima are A + B and A ~ B; B represents the mean value for equal angular spaces round the primary.

The annexed Table contains the values of the more important quantities for most of the orbits whose elements are known. Columns 5 and 6 contain the maxima values of p W, and include both magnitudes and signs. In column 8 are found the angles of position corresponding to the points for which W vanishes. The symbol + between the two angles indicates that, for positions of the secondary between the minus, W is positive. The reverse is denoted by the symbol —.

TABLE I.

Star.	Elements computed by	A.	B.	Greater max. value of p W.	Less max. value of p W.	Log A.	Angles of Position for which p W = 0.
							° ′ ° ′
η Coronæ Bor. . .	Mädler . .	·92646	·04704	− ·97350	+ ·87942	$\bar{1}$·966 8285	105 22 − 303 14
ζ Cancri . . .	Mädler . .	·7821	·1281	+ ·9102	− ·65395	$\bar{1}$·893 2505	71 11½ + 291 44½
ξ Ursæ Majoris . .	Herschel .	1·9528	·4097	+ 2·3625	− 1·5431	·290 6615	153 44 + 41 50
ω Leonis . . .	Villarceau .	·3608	·2311	− ·5918	+ ·1297	$\bar{1}$·557 2011	174 41½ − 95 40½
p Ophiuchi . .	Herschel .	1·6851	·6502	+ 2·3353	− 1·0349	·226 6103	184 52 + 69 58
ξ Boötis . . .	Herschel .	4·8088	2·8552	− 7·6640	+ 1·9536	·682 0362	166 58 + 193 7
δ Cygni . . .	Hind . .	·3381	·09184	+ ·4300	− ·2463	$\bar{1}$·529 0691	91 39 + 337 9
γ Virginis . . .	Herschel .	·7002	·4566	− 1·1568	+ ·2436	$\bar{1}$·845 2215	183 52½ + 317 43½
Castor . . .	Herschel .	1·6903	·1669	+ 1·8572	− 1·5234	·227 9533	96 52 + 309 18
σ Coronæ Bor. . .	Hind . .	·1624	·04147	+ ·2039	− ·1210	$\bar{1}$·210 6979	127 22½ − 274 43½
μ_2 Boötis . . .	Hind . .	·2445	·04719	+ ·2916	− ·1973	$\bar{1}$·388 1834	191 16½ + 43 25½
α Centauri . .	Jacob . .	17·533	6·0684	+ 23·6014	− 11·4646	1·243 8514	204 57½ − 327 16½
α Centauri . .	Powell . .	12·433	6·2070	+ 18·640	− 6·226	1·094 5392	9 27½ + 39 8½
ζ Herculis . . .	Villarceau .	·9765	·1127	+ 1·08914	− ·8638	$\bar{1}$·989 6616	115 13 + 313 29

The great preeminence of *a Centauri* in this list is due, mainly to the largeness of its mean distance caused by its proximity to us. With the aid of the commonly received value of its parallax, I have calculated the two maxima values of W and its value at the present epoch. They are given at the commencement of this paper.

§ 4. Through the kindness of Mr. Gill, who has supplied me with some of Dembowski's recent measurements of binary systems, I am enabled to add the present values of p W in certain cases. The observations do not give directly the values of the distance and position-angle at the present time; but we may approximate to them by assuming their rates of change, as determined by two sets of observations, to remain sensibly constant for a few years. In two cases the observations of Dembowski have been combined with those of Brünnow, made at Dunsink during the years 1868-69.

The distances are given in the table to indicate what stars are more favourably situated for observation:—

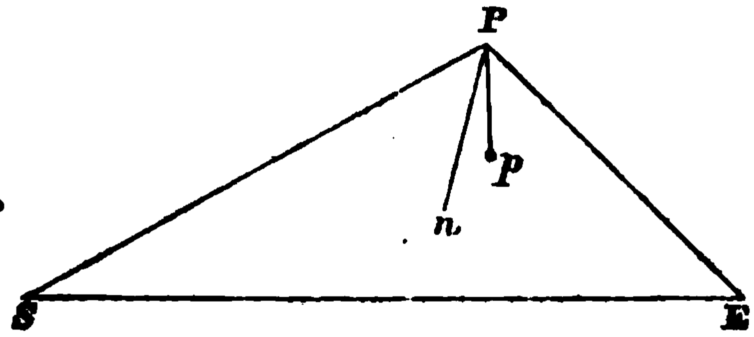
TABLE II.

Star.	Position-Angle and Distance.							Value of p W.
	Observed.				Calculated.			
	Epoch.	Position-Angle.	Distance.	Observer.	Epoch.	Position-Angle.	Distance.	
		°	"			°	"	
η Coronæ Bor.	1872·44	50·86	...	D	1874·44	61·44	0·975	+ ·3167
	1873·44	56·15						
ζ Cancri	1872·33	162·84	...	D	1873·33	135·86	1·0	+ ·4432
	1873·33	150·23	0·5					
ξ Ursæ Majoris	1872·33	19·39	...	D	1873·33	338·43	·882	+ 1·0078
	1873·33	358·91						
p Ophiuchi	1868·90	98·008	4·92	B	...	...	...	+ ·4565
δ Cygni	1868·69	349·06	1·52	B	...	...	...	+ ·09307
γ Virginis	1869·22	164·98	...	B	1873·33	159·01	4·52	− ·4757
	1872·86	160·79	...	D				
Castor	1869·16	238·97	5·71	B	...	...	...	− ·86162
σ Coronæ Bor.	1868·55	194·048	...	B	1873·33	199·33	3·13	− ·12087
	1872·96	198·08	...	D				
ζ Herculis	1872·48	173·88	...	D	1873·33	153·62	1·43	+ ·4893
	1873·52	162·49						

§ 5. *Displacement of the Lines in the Spectrum of a Planet.*—In all the cases, so far as I am aware, in which the effect of the motion of the source of light on the spectrum lines has hitherto been treated, the body has been supposed self-luminous. But it may be readily shown that somewhat similar results obtain when the substance shines either by reflected or transmitted light. To make this clear in the case of reflexion, which is the more important one from an astronomical point of view, let us first consider what would happen if the spectator and source of light were in a line with the reflecting body, and the latter were moving towards us with a velocity v. Supposing any change of phase which may occur at reflexion to be unaffected by the motion of the mirror, it follows that the path of a second luminous crest will be shorter than that of the first by twice the distance which the mirror has travelled during the period of a single undulation. The number of vibrations per second in the reflected light is therefore increased in the ratio $1 + \frac{2v}{V} : 1$, where V is the velocity of light. The wave-length of the light which the spectator receives is shortened in the same ratio.

The effect of the mirror in this case is, thus, double what would take place if the body were self-luminant.

Suppose now, in the second place, that the incidence on the reflector is oblique, and that the latter is moving in any direction in the plane of incidence. Let S be the source, E the spectator, P the reflector, P p the distance moved in time $\delta t = v \delta t$, P n the bisector of the angle S P E. Let also S P E $= 2\alpha$, p P $n = \beta$.



Since it is a theorem that, for the point (P) of reflexion at the mirror, S P + E P is a minimum, and therefore stationary, we may still in the new position of the mirror treat p as the point reflexion. By the motion of P the path of the light is shortened by a length $v\,\delta t\,(\cos\overline{\alpha + \beta} + \cos\overline{\alpha - \beta}) = 2\,v \cos\alpha \cos\beta\,\delta t$. The period of vibration and wave-length are, thus, each shortened by a fraction of itself $= \frac{2\,v \cos\alpha \cos\beta}{V}$. The reverse would happen if P moved in the direction p P.

[A very similar formula expresses the effect of any motion possessed by a refracting surface; and it follows that, in transmission through a plate with parallel faces, motion of the latter produces no effect whatever on the lines of the spectrum.]

§ 6. These principles may be applied to the analysis of the light which we receive from the planets. And in showing how this may be done, I shall adopt the restriction that all the planets revolve in the ecliptic, in concentric circles round the Sun.

The results which we shall thus obtain will not be strictly accurate, but will probably not differ much in their general features from those actually presented in nature. They will, at least, enable us to judge, with more or less truth, of the magnitudes of the numbers with which we have to deal.

Since the magnitudes with which we are concerned are exceedingly minute, we may employ the principle of the superposition of small changes to separate the effect due to the motion of the planet from that of the Earth. Let these bodies be represented as in the former figures by P, E, and let their distances from the Sun be a, b miles, and let their times of revolution be T and t years. P and E are moving at right angles to S P, S E with velocities $\frac{2\pi a}{Tn}$ and $\frac{2\pi b}{tn}$ miles per second, the direction of revolution being from E to P. Denoting these by u, v, it is evident that the joint effect of the motion of both is to increase the wave-length of the light emitted from the Sun by a fraction of itself = $\frac{U}{V}$, where

$$U = 2u \cos\alpha \sin\alpha - u \sin SEP$$

$$= \frac{2\pi}{n}\left(\frac{1}{T} - \frac{1}{t}\right) b \sin SEP \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

From this formula we gather the general result that, for equal angular distances of a planet for the Sun, the values of U are numerically equal. To investigate it more narrowly, two cases must be distinguished,—

1°. For an *inferior* planet, for which $t > T$, U is positive when the planet is east of the Sun, and negative when west of it. At the greatest elongations of a planet U is a maximum and numerically $= u\left(1 - \frac{T}{t}\right)$.

2°. For a *superior* planet, for which $t < T$, U is negative for positions east of the Sun, and positive for western positions. Its maximum is reached when the distance is a right angle, and it then amounts to $u\left(1 - \frac{t}{T}\right)$.

The following Table gives the values of U_g for the principal planets calculated with the restrictions above mentioned:—

TABLE III.

Planet	U_g in miles per second
Mercury	22·24
Venus	8·24
Mars	8·57
Asteroids	13·87
Jupiter	16·76
Saturn	17·68

Queen's College, Cork,
1874, *April* 11.

On a piece of Apparatus for carrying out M. Janssen's Method of Time-Photographic Observations of the Transit of Venus. By Warren De La Rue, D.C.L., F.R.S.

(*Described verbally at the Meeting of the* 10*th of April*, 1874.)

It will be remembered that M. Janssen proposed to take advantage of the preparations which were being made for photographic observations of the transit of *Venus*, by adding thereto a special contrivance by means of which a series of photograms might be taken in rapid succession, when *Venus* had actually entered on the limb of the Sun. It was thus hoped to ensure a more exact time-record of the internal contacts of the planet and the limb of the Sun, than could be obtained by eye-observations.

The general scheme of the apparatus necessary to effect this object was well considered by M. Janssen, and there is little novelty in the form of the instrument I am about to describe; nevertheless, as there are some peculiarities in its construction, and as it contains the fewest possible number of moving parts, I have thought that it might not be unworthy of the notice of the meeting.

The photographic plate to be employed is supported in a cell which rotates on an axis excentric to the axis of the telescope;

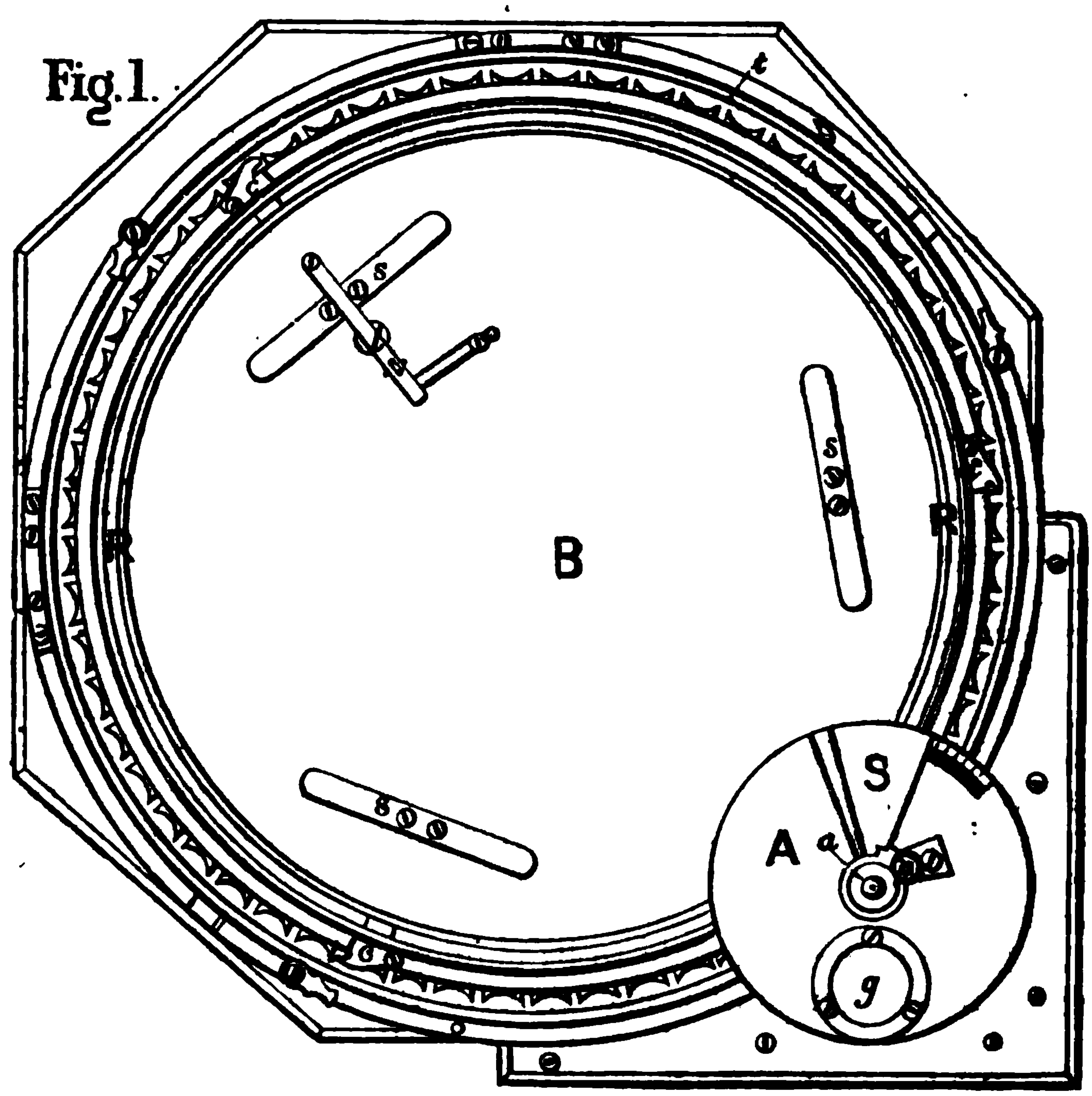
in the cover of the fixed frame in which the cell rotates is a radial slit near the centre of the telescope and exposing a portion of the zone (one-sixtieth of its circumference); just above the cover of the plate-holder is a circular revolving shutter with an adjustable radial slit, the axis of this shutter is just beyond the periphery of the cell, and is therefore excentric to the cell; it carries a pin which falls into one of sixty radial slots racked in the edge of the plate-holder and carries it round one step for every revolution it makes. This axis, during each revolution, exposes one-sixtieth of the sensitive plate, then shuts off the light, and lastly brings a fresh portion of the film into position for the succeeding photogram.

Each time the instantaneous shutter exposes the plate contact is made with a wire from a voltaic battery and an electric circuit established with the electro-magnet of a chronograph.

The sensitive plate to be used is a disk somewhat less than 11 inches in diameter; the fixed frame or casing is about 13 inches in its largest dimension and $2\frac{1}{4}$ inches thick: the whole apparatus weighs twelve pounds and a quarter including the glass disk.

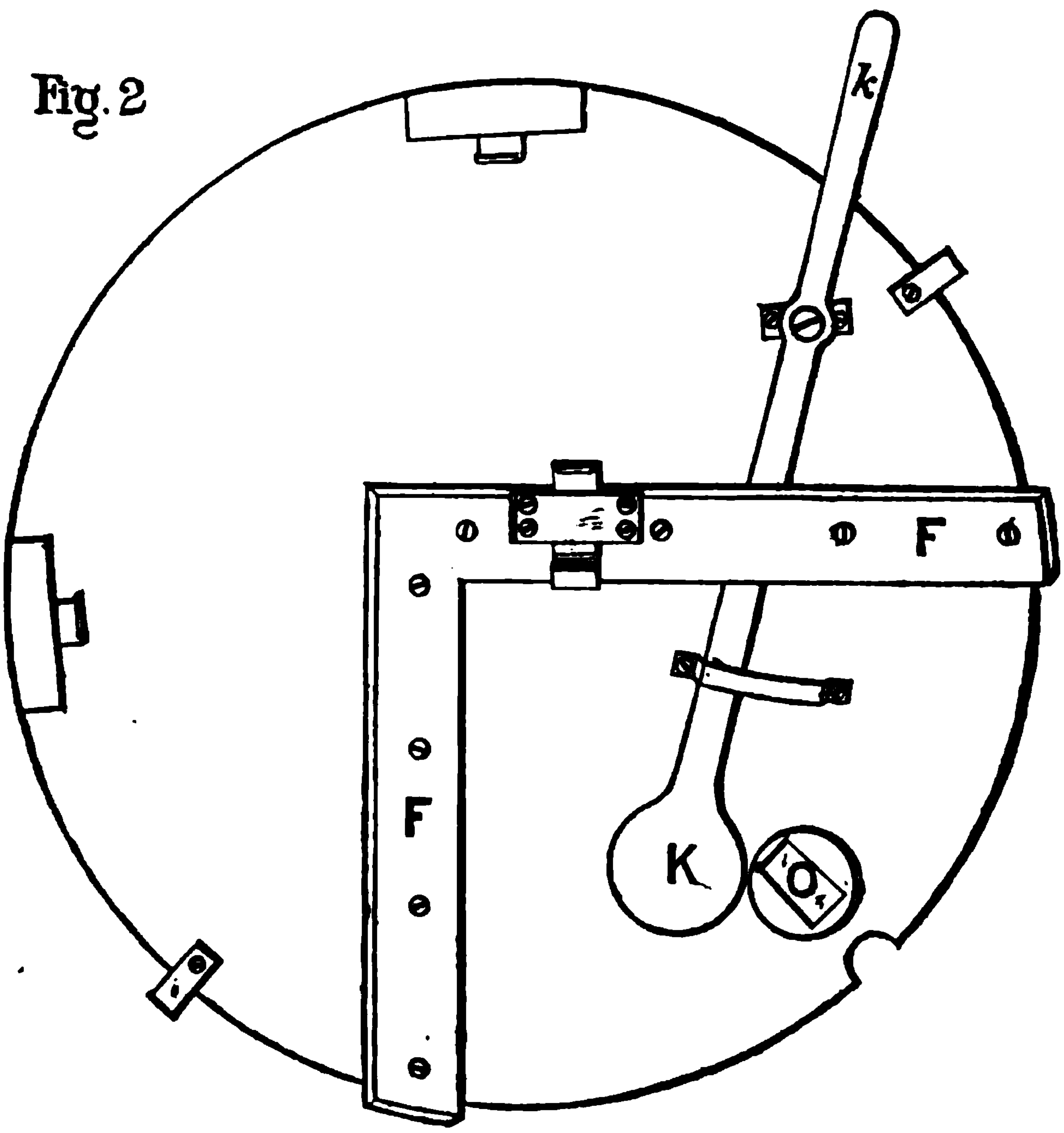
Fig. 1 shows that face of the apparatus which is turned towards the secondary magnifier when it is placed in the photoheliograph; the cover, fig. 2, having been removed to show the several parts.

Fig. 1.



B, fig. 1, is the bottom plate of the circular cell which holds the glass disk; it is racked with 60 radial slots *t* and 60 circular spaces (these are shown in full size in fig. 4), *c c c* is a ring screwed to B to form the cell, it is electro-silver plated on the inside to prevent the nitrate of silver from acting upon it if wet plates are used. A removable ring R R, also electro-plated, serves to hold the glass disk in a plane at right angles to the axis of the telescope; it can be taken out to permit of the insertion of the glass disk after turning aside the catches *c c c*. The disk may then be dropped (the sensitive film uppermost) gently into its place on three pressure springs *s s s*, and the ring R R pressed on it; the catches *c c c* when turned over the ring keep it exactly in its proper place, as they are bevelled at the edge and slide under

Fig. 2



dovetailed recesses in the rim of the cell. The cover shown in fig. 2 must then be placed over the plate and secured by the bolts and catches shown in fig. 1; the radial opening O must, of course, be covered by the shutter K before the apparatus is taken to the photoheliograph.

A, fig. 1, shows the instantaneous apparatus: it is a circular disk 4 inches in diameter, having a radial opening of about 1 inch at the periphery; the aperture is provided with a shutter by which

it may be completely closed, or the shutter may be opened to any extent by turning the pinion *p* which works into a portion of the shutter toothed for that purpose; the adjustment is facilitated by a graduated arc shown in the figure. On the opposite side there is inserted a disk of so-called ruby glass (copper-red) 1 inch in diameter; in a certain position of the cell this permits of the Sun's image being seen and adjusted to its proper place.

Fig. 3.

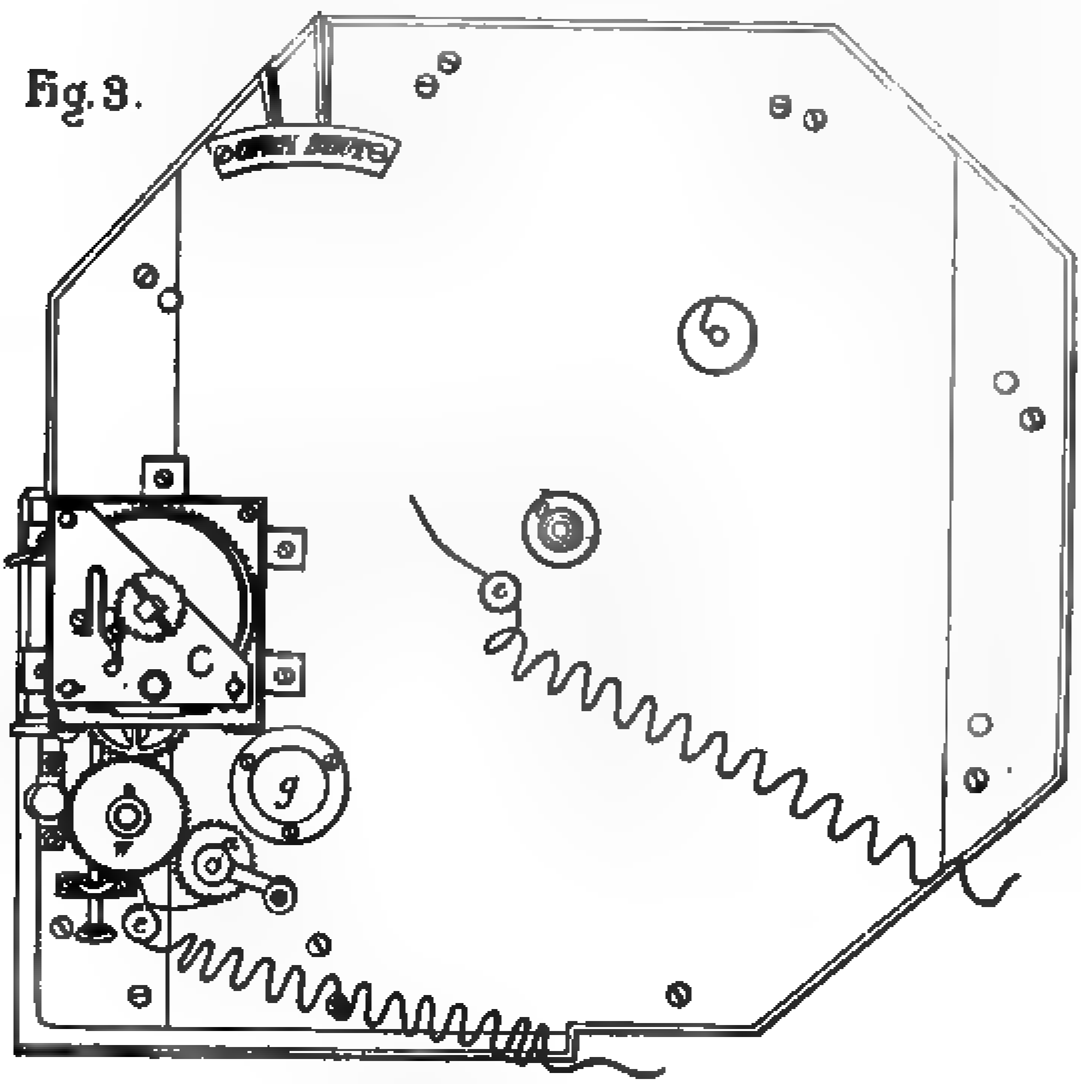


Fig. 3 represents the back of the apparatus; *b* is the axis of the plate-holder B; *a* is the axis of the instantaneous shutter; on it is fixed a toothed wheel to connect it with the clock-work driver, and also a handle to permit of its being turned by hand. *w* is an intermediate wheel always in gear with the wheel on *a*, but attached to an arm moving round *a* which permits of its being thrown in and out of gear with the driving-wheel of the clock C. In preparing the apparatus for work, *w* is thrown out of gear and the handle turned from left to right a sufficient number of revolutions to bring it to a stop, provided for that purpose; the stop is formed by racking the last of the slots *tt* for only a small distance.

The sensitive plate is now put into its cell, as before de-

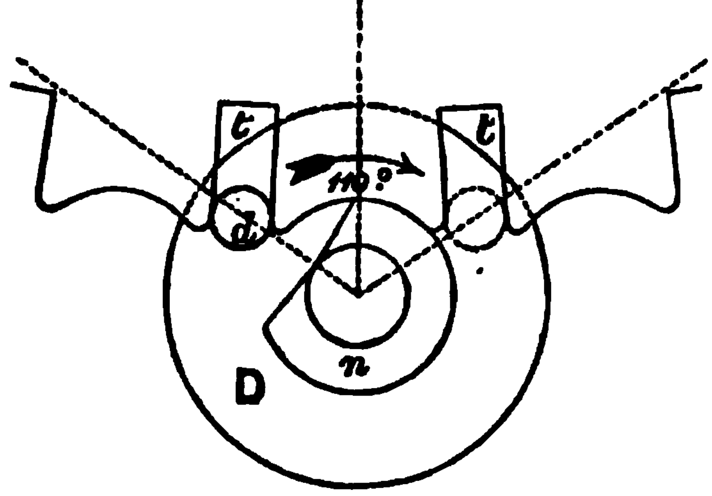
scribed, and the apparatus attached to the photoheliograph by means of the framework, F F fig. 2, fixed to the moveable cover; a screw-bolt in the photoheliograph fastens the framing firmly in its place. Attention having been paid to the directions given in the preceding paragraph, the aperture *g* covered with red glass will correspond exactly in position with similar red glass covered apertures in the instantaneous shutter and the bottom of the plate-holder B, and the Sun's image can be then seen and adjusted in position when the handle *k* of the shutter K has been set to the word "open."

The wheel *w* has then to be geared with the clock-driver C (previously wound up and stopped by a small detent, which acts against the fly governor). All being now ready, and the signal having been given, the detent, shown in fig. 3 on the left at the top of the clock-driver, is released with a slight touch of the finger and the apparatus starts and takes 60 successive photograms at any required interval; suppose at intervals of a second or of two seconds. A regulating pressure screw permits of the interval being varied within certain limits. After 60 photograms have been taken the apparatus stops because the pin *d*, fig. 4, enters the short slot.

On the axis *a* of the instantaneous shutter is fixed a disk of ivory in the periphery of which is inserted a platinum pin screwed into the pinion *a*; an insulated spring tipped with platinum presses against the edge of the ivory disk and completes the circuit of a voltaic battery connected with the chronograph at the exact instant that the slit in the shutter exposes a portion of the sensitive plate. Thus, even if the movement is not transmitted with perfect regularity, the precise epoch of each photogram will, nevertheless, be recorded.

It only now remains to describe the very simple mechanical contrivance by means of which a sixtieth part of the sensitive plate is successively brought into position under the radial

Fig. 4.



opening O, fig. 2, in the cover of the apparatus. Referring to fig. 4 drawn full size, D is a disk having a boss *n*, it is fixed on the axis *a* of the instantaneous shutter; in D is fixed a pin *d*

which, once in every revolution of the axis *a*, enters into one of the radial slots *t* of the plate-holder and carries it round one-sixtieth part of a revolution, reaching then the position shown by the dotted circle after having passed through an arc of 110°. Before it leaves the slot, the boss *n* has passed into one of the circular recesses corresponding to its own curvature, and locks the plate-holder firmly. In order to permit of the rotation of the plate-holder at the proper time, a portion of the boss *n* is cut away to a sufficient extent, as shown in the figure.

The apparatus just described has been fitted up for the Indian photoheliograph.

I may here state that Mr. Christie and M. Dallmeyer have contrived another form of apparatus also founded on M. Janssen's general proposition; and I will now describe another modification (verbally described at the meeting May 9th), which in reality is the one which I first devised. It was designed in order that it might exactly fall within the channels provided in the photoheliographs for the plate-holder to contain plates 6 in. × 6 in.; the conditions were more difficult to comply with than in the before-described plan, and the apparatus is conse-

Fig. 5.

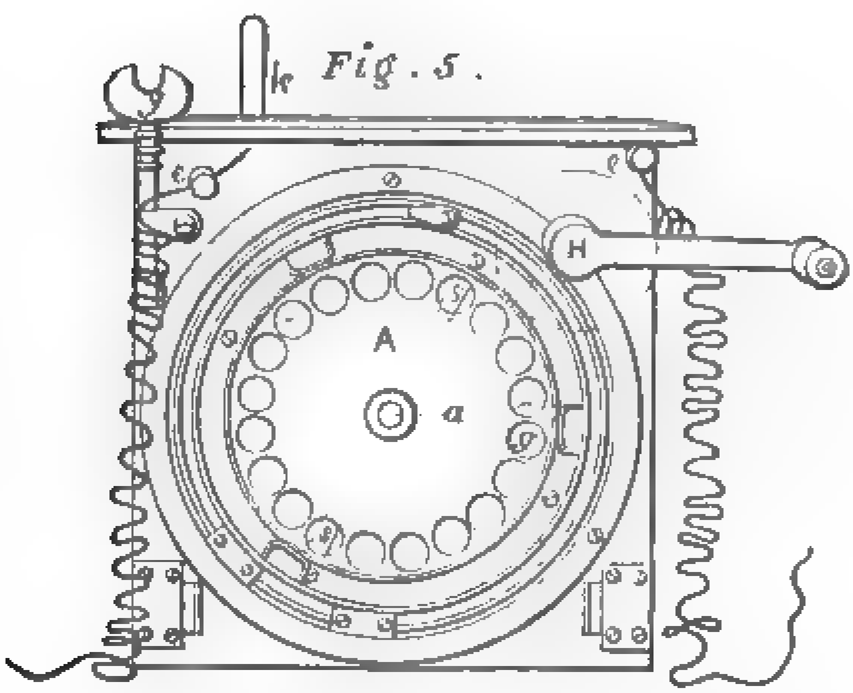
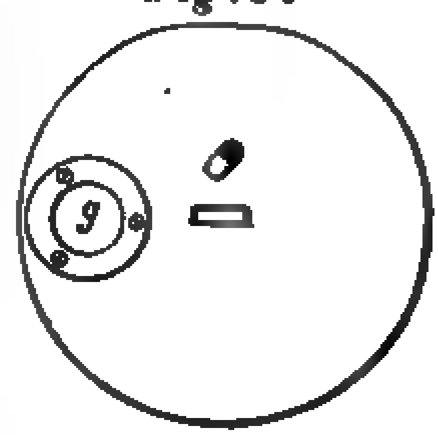


Fig. 6.

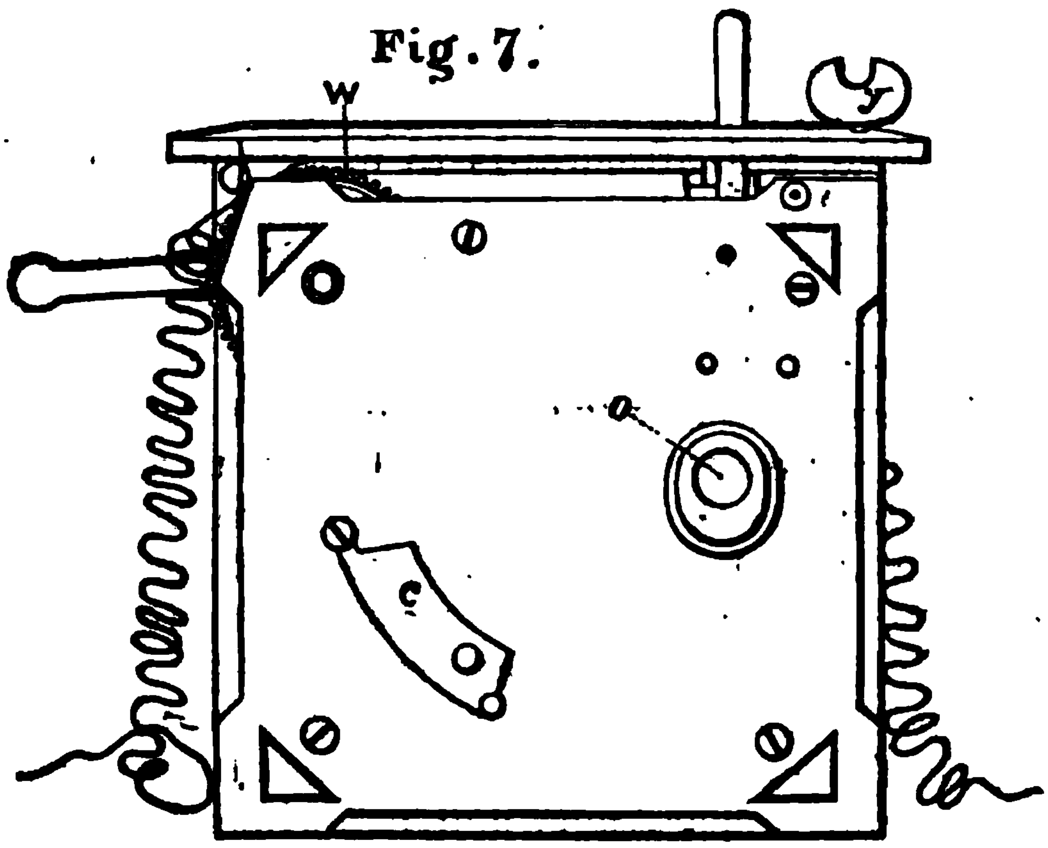


quently somewhat more complicated. Fig. 5 shows the back of the apparatus, that is the side turned away from the secondary magnifier. The cell A which holds the plate is perforated with twenty circular apertures through which the image of a portion of the Sun will be formed; it and the instantaneous apparatus rotate on the stud *a*; the sensitive film in this apparatus is placed downwards and rests on three bridle-wires of silver against which the plate is pressed by means of springs at the back of the cover fig. 6; the cover in this apparatus rotates with the cell to

which it is fastened by clips. There is a piece of red glass *g* inserted in it to permit of the Sun's image being seen; it corresponds in a certain position of the cell with similar pieces, one of which is inserted in the instantaneous shutter, the other in the shutter *k*.

The instantaneous shutter is concentric with the cell which holds the plates, and has two adjustable radial apertures *s s* Fig. 5,

Fig. 7.



which can be adjusted by means of the spanner *y*, after turning aside the door *c*; it is so geared with the wheel W on the axis of the handle H, fig. 7, that it makes half a revolution for one turn of the handle. On the axis of the handle is a pin which once in every revolution takes into one of the twenty epicycloidal teeth racked in the edge of the plate-holder, and between these teeth are circular locking recesses for a boss on the axis to enter and to lock the plate in position. *o* is an aperture which permits of the Sun's rays passing through one of the openings in the plate-holder.

To prepare the apparatus for work the handle H has to be turned from right to left a sufficient number of times to bring the driving-pin to the stop; the red glasses are then exactly one over the other. The handle must be turned in a contrary direction (left to right) to produce the photograms until after twenty pictures are obtained the pin again comes against the stop. There is a provision *ee* in the apparatus for electric communication with a chronograph. This particular apparatus is destined for the Melbourne Photoheliograph now in course of construction by M. Dallmeyer, as there is not sufficient time to provide one for sixty photograms.

On an Apparatus designed for the Photographic Record of the Transit of Venus.

By Signor J. Capello.

(*Extract from a letter to Dr. De La Rue.* Translation.)

I have just received your precious letter, with the sketch of the apparatus for M. Janssen's process. It is the only method of obtaining the exact instant of the contacts, free from personal errors, &c., in order to calculate the parallax by the methods of Halley or Delisle, and afterwards to make the comparison with the photographic methods properly so called, that is to say, the measurement of the series of photograms obtained after the first and before the last internal contacts. I have also received Mr. Abney's process: I was already acquainted with it from the *Photographic Year-book* of 1874, but I had not yet used it.

It is with the greatest regret that I have to announce to you that our expedition has failed. Our government, after having approved (May 1873) the plan and budget proposed by our Academy of Sciences: after having spent more than 350*l.* in preparations of all kinds, has recently (10 April 1874) abandoned the plan, for want of authority from the Chambers to pay the costs of the expedition: these costs do not exceed 2,000*l.*

We had made preparations and experiments for proceeding on the following plan. The difficulties were vanquished.

The dome or tower of the photoheliograph was itself a camera obscura: there was in a moveable shutter a hole always covered by the instantaneous apparatus: and another with a red glass, in order to point the photoheliograph by means of a lens at the side of the tube; this lens acting as a finder.

The instantaneous apparatus is quite independent of the photoheliograph; it is on an opaque shutter upon the moveable roof covering the hole *A* which allows the passage of the solar rays at the instant of the impression: this apparatus is composed of a cardboard disk, with a cut-out sector, figs. 1 and 2.

Fig 1.

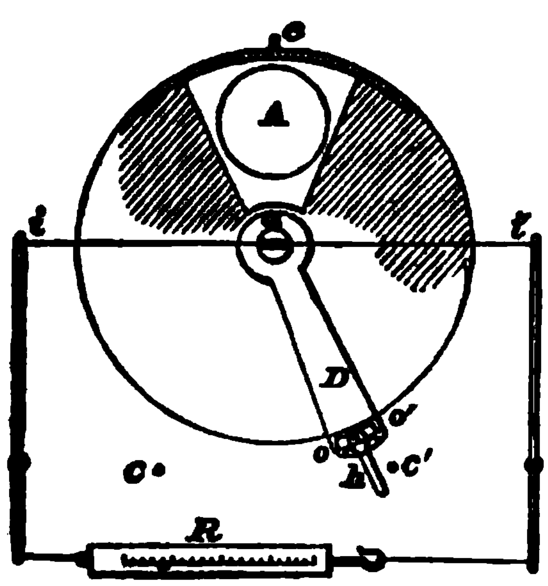
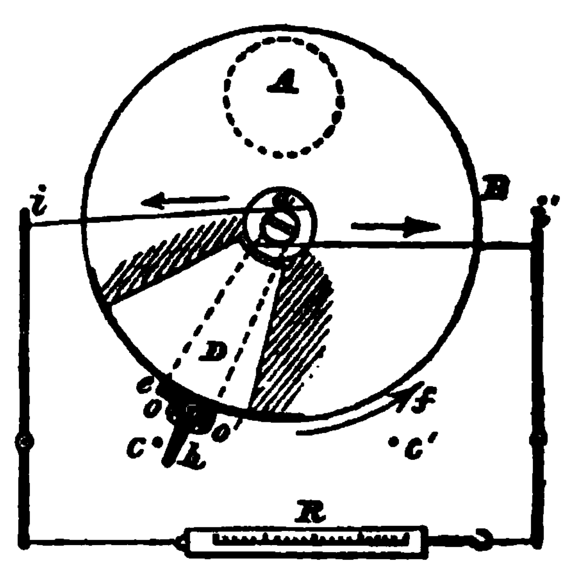


Fig. 2.



The axis (a) is pierced with a hole through which passes a thread $i\,i'$, which is kept stretched by the spring R by means of two vertical levers. In the figure 1 the apparatus is at rest; by making it assume the position of figure 2, moving in the direction $A\,B\,e$ until the tooth e catches in the click h, it is drawn forcibly bac k by the spring, and tends to return in the contrary direction to a point near c'. Moving the arm D to which is fixed the click h, until it presses against the fixed point c the tooth e escapes, and after making a whole revolution the tooth e again catches, but on the side c'. Making the pointer move subsequently from c towards c', and after having touched h at c', the disk describes another revolution, but in the contrary direction, and so on alternately (from left to right and from right to left).

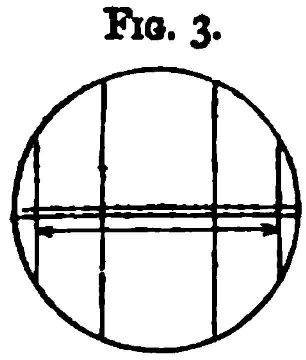
To each revolution corresponds an exposure. The duration of the exposure can be regulated, to within some thousandths of a second, to 0·4 or 0·5 of a second, according to the tension of the spring R.

The whole of this apparatus is the invention of one of the members chosen for the observations, M. Campos Rodrigues, of the Astronomical Observatory.

The dome of the photoheliograph being dark, the dry plates can be handled perfectly without the aid of the plate-holder (slide): the plates resting on three silver points by means of a simple system of supports, also contrived by M. Campos Rodrigues. By this method one can, without difficulty, expose three plates in every two minutes, or even more rapidly.

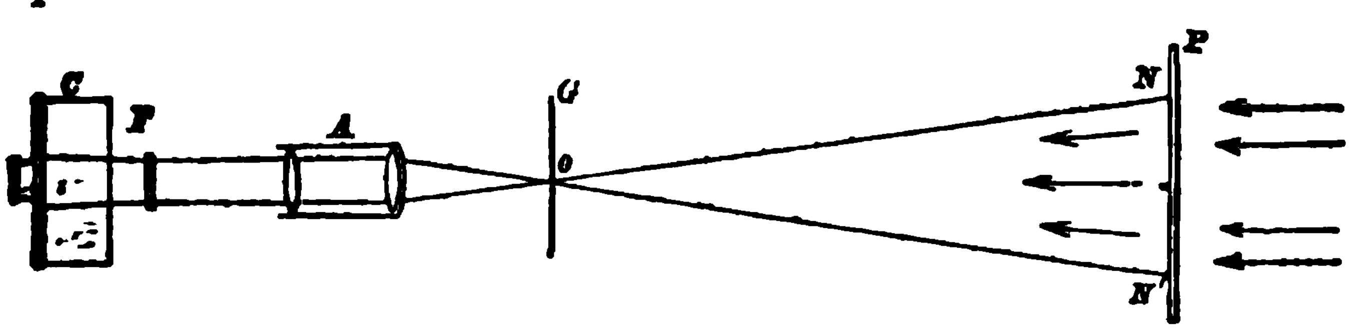
The dry collodion we had fixed upon was that chosen by the German Commission, and made by Dr. Fogel of Berlin. The process is Fothergill's albumen-process, with slight modifications; it has given me very good images of the Sun; and according to Vogel's experiments, verified by myself, it contracts very slightly.

FIG. 3.



We have had spider threads on a very exact micrometer placed in the focal plane of the object-glass: the two horizontal ones being in the direction of the celestial equator, and the others perpendicular to it. The distance between the exterior ones carefully measured by the times of star transits would be our unity for measurement of the photograms.

The distortion always met with in the secondary magnifiers would be quite eliminated by the method of measuring the images of the photograms, due to M. Campos Rodrigues. The principle is quite astronomical.



In the measurements we employ the same magnifier A, and in the same relative position which it had in respect to the plate P on the occasion of the impression.

Making the luminous rays describe a path exactly opposite to that which they had in producing the picture, it is evident that the rays which have traversed the plate, proceed within the cone $O\,N\,N'$ (the others are cut off by the diaphragm G at the optical point o), and traverse the lenses of the magnifier through the same points of the lenses which they had traversed in their path through the plate; and, consequently, in this second passage are modified equally, but in the contrary sense, so that the new image at F is exactly equal to the focal image of the object-glass, the effects of distortion thus disappearing. Placing a micrometer at O, we can make the measures as in the direct observation of the Sun.

It is obvious that it is quite a different thing to measure the image of the Sun thus diminished by the magnifier, or to measure a photographic image produced by the object-glass; in this second case all the imperfections are augmented; one magnifies the granulation of the collodion, or of the silver, &c.; which does not happen in the first case.

These are the principal points of the method we had decided upon, and which unfortunately, will not be realised; it is just this which I regret, not to be able to realise a plan which in all probability would have succeeded.

If you approve of any of these points, for instance the method of measuring the photograms, it might be possible to employ them at some of the English stations.

Lisbon,
1874, *April* 26.

Signor Capello, in a subsequent letter, enquires whether the *personnel* of the different English expeditions is quite filled up; and, in the contrary case, whether it would be possible for him to join in any one of them; by preference in the Northern hemisphere, so as not to take him too long a time from the Observatory. He has practised magnetic observations for sixteen years.

On the Lunar Semi-diameter derived from Occultations of Stars. By E. Neison, Esq.

It has been shown from a reduction of the Greenwich observation for the years 1861–1870 that the semi-diameter given by occultations of stars at the dark limb was considerably less than Hansen's value. As a variation in the apparent semi-diameter arising from irradiation was found with instruments of different aperture, it became necessary to determine if a similar variation existed in

the correction from occultations. For this purpose the observations made at the Radcliffe Observatory with an aperture of $7\frac{1}{2}$ inches during the period 1862–1872, and those made at Cambridge with a mean aperture of 11 inches during the period 1861–1869, were compared with the Greenwich observations for 1861–1871, made with an average aperture of about 6 inches.

Seeing the great difficulty in making the observations, the small number made, and the one-sided nature of the errors, the determinations from the re-appearances are omitted.

	Disappearances at Dark Limb.	No. of Obs.	Disappearances at Bright Limb.	No. of Obs.
	"		"	
Greenwich	−1·73	42	+1·06	12
Radcliffe	−1·81	30	+0·52	13
Cambridge	−1·95	14	+0·01	3

The correction for the first class is nearly constant; while the gradually decreasing correction with increase of aperture in the second class, is exactly what is to be anticipated. It is evident that only in the disappearances at the dark limb is the full effect of the correction apparent; for at the disappearances at the bright limb, owing to the great comparative faintness of the star, there is a considerable tendency to lose sight of it before actual occultation. The very great difficulty in observing the reappearances at the exact instant of becoming visible, and not a few seconds later, tends, like the one-sided nature of the resulting error, to increase the real occultation semi-diameter, and so lessen very much their value.

Combining now the whole series of 176 observations, we have, as the correction to Hansen's semi-diameter:

	Correction.	No. of Obs.
	"	
Disappearances at the Dark Limb	−1·80	86
„ „ Bright Limb	+0·70	28
Reappearances at the Dark Limb	+0·13	29
„ „ Bright Limb	+1·84	33

Owing to the irradiation at the lunar limb, the actual mean diameter is not known; but the minimum value it can have, is found from observations of Solar eclipses where the effects of irradiation are reversed.

As the effect of irradiation appears to vary directly as the difference in brilliancy of the two objects, it is to be expected that its effects would be greater in the case of the dark Moon on the Sun's disk than in that of the bright Moon against the sky. Assuming that of the difference observed between the diameters of the bright and of the dark eclipsed Moon a little over one-half is due to solar irradiation, as the contact is then greatest, the minimum

lunar semi-diameter can be found. For the observations of the Eclipses of July 18, 1860, and December 21, 1870, at Greenwich with the great Equatoreal, give as the minimum value for the mean lunar semi-diameter, Hansen's value less 0″·13 and less 0″·20 respectively. This value being still over 1″·60 greater than the occultation semi-diameter, it would appear that irradiation fails to account for the difference found to exist.

To compare these results with those obtained by the Astronomer Royal (*Greenwich Observations*, 1864), they were reduced to the same parallax and semi-diameter there used. As the earlier observations of the series employed by the Astronomer Royal are by no means so good as those made at later periods, when the Altazimuth has been in use, they are omitted. Employing then those made during the period 1850–1860, with an average aperture of a little over four inches, and comparing with the results of the later Greenwich, and the Radcliffe and Cambridge observations, we have, for the disappearances at the dark limb:

	″		″
Greenwich, 1850–1860 =	−2·45	Greenwich, 1861–1871 =	−2·87
Radcliffe, 1862–1872 =	−2·81	Cambridge, 1861–1869 =	−2·87

Finally combining the whole four series, we have, as reliable values, from 303 observations, reduced from good lunar places, and with fine instruments, for the correction to the telescopic semi-diameter employed by the Astronomer Royal:

		″		
Disappearances at the Dark Limb	=	−2·74	from	143 obs.
„ „ Bright Limb	=	−1·29	„	46 obs.
Reappearances at the Dark Limb	=	−1·08	„	58 obs.
„ „ Bright Limb	=	+0·46	„	56 obs.

London,
1874, *30th March.*

The Solar and Planetary Systems. By Maxwell Hall, Esq.

The author states Bode's law as follows:—"In the solar and planetary systems the mean distances of the planets and satellites do not greatly differ in value from the terms of the series:

$$4\lambda,\ 7\lambda,\ 10\lambda,\ 16\lambda,\ 28\lambda,\ 52\lambda,\ 100\lambda,\ 196\lambda,\ 388\lambda,\ \&c.,$$

where λ has different values in different systems. But there may be more than one, or there may be no planet or satellite near any of the above theoretical distances." And he then proceeds to determine λ in miles for the planetary system, and for the Jovian, Saturnian, and Uranian satellite systems respectively.

Some of the numerical coincidences are very close, thus in the Uranian system, taking the distances to be 7λ, 10λ, 16λ, and 28λ, the first three satellites give $\lambda = 17600$, 17100, and 17600 miles respectively, (but the fourth satellite gives $\lambda = 13400$ miles).

He then states a second proposition: "Twice the unit of length in any system is approximately equal to that distance which corresponds to the period of rotation of the central body of that system," or say

$$\lambda = 1580\, M^{\frac{1}{3}}\, P^{\frac{2}{3}}$$

where $M =$ mass of central body in terms of the mass of the earth, P period of the axial rotation in hours, λ in miles as before. It thus appears that dividing the value of λ for any system by the value of $M^{\frac{1}{3}} P^{\frac{2}{3}}$ for the central body of that system, the quotient should be 1580. For the Solar, Jovian, and Saturnian systems, the quotients are 1790, 1340, 1720, mean 1620. For the Earth $\lambda = 13100$ miles; so that regarding the Moon as a fourth satellite (the three interior ones missing) its theoretical distance is 210,000 miles. The paper concludes with some considerations as to M. Lescarbault's planet *Vulcan*.

Memorandum of Observations of Jupiter made during the month of April 1874. By John Brett, Esq.

I wish to call the attention of the Fellows to a particular feature of *Jupiter's* disk, which appears to me very well defined at the present time, and which seems to afford evidence respecting the physical condition of the planet.

The large white patches which occur on and about the equatoreal zone, and interrupt the continuity of the dark belts, are well known to all observers, and the particular point in connection with them, to which I beg leave to call attention is, that *they cast shadows;* that is to say, the light patches are bounded on the side farthest from the Sun by a dark border shaded off softly towards the light, and showing in a distinct manner that the patches are projected or relieved from the body of the planet.

The evidence which this observation is calculated to afford refers to the question whether the opaque body of the planet is seen in the dark belts or the bright ones, and points to the conclusion that it is not seen at all in either of them, but that all we see of *Jupiter* consists of semi-transparent materials.

The particular fact from which this inference would be drawn is, that the dark sides of the suspended or projected masses are not sufficiently hard or sharply defined for shadows falling upon an opaque surface, neither are they sharper upon the light background than upon the dark.

The laws of light and shade upon opaque bodies are very simple and very absolute; and one of the most rudimentary of them is, that every body has its light, its shade, and its shadow, the relations between which are constant; and that the most conspicuous and persistent edge or limit in this association of elements is the boundary of the shadow; the shadow being radically different from the shade in that its intensity is uniform throughout in any given instance, and is not affected by the form of the surface on which it is cast, whereas the shade is distinguished by attributes of an opposite character.

Now if the dark spaces adjoining the light patches on *Jupiter*, which I have called shadows, are not shadows at all, but shades, it is obvious that the opaque surface of the planet on which the shadows should fall is concealed; whereas if they are shadows, their boundaries are so soft and undefined, as to lead to the conclusion that they are cast upon a semi-transparent body, which allows the shadow to be seen indeed, but with diminishing distinctness towards its edge, according to the acuteness of its angle of incidence.

Either explanation of the phenomenon may be the true one, but they both lead to the same conclusion, viz., that neither the dark belts nor the bright ones are opaque, and that if *Jupiter* has any nucleus at all, it is not visible to us.

It is obvious that the phenomenon I have described would not be visible at the time of the planet's opposition, and the first occasion on which I noticed it was the night of the 16th of April last. The drawing* which accompanies this memorandum represents that particular observation; but since that date I have seen it even more distinctly on several occasions, and I venture to remark, that the time of opposition may prove to be as unfavourable for examining *Jupiter* as it is for the Moon.

The instrument used was a $9\frac{1}{4}$-inch silvered reflector, with achromatic eye-pieces; the power usually found most effective being 400.

By the kind invitation of Mr. Lassell, I had an opportunity on the 20th of April of examining the disk with his 20-foot reflector of 24 inches aperture, and I found this large instrument confirm my impressions concerning the shadows in the most satisfactory manner.

38 Harley Street, W.

Bright Spots on Jupiter. By Joseph Gledhill, Esq., F.G.S.

These curious and beautiful objects were finely seen here about midnight, April 23rd. They lay just within the shading which surrounded the south pole of *Jupiter*. Only three were seen. They seemed quite round, about the size of Sat. I., when fairly

* This drawing was exhibited at the Meeting.—Ed.

within the disk, were sharply defined, and quite as bright as any of the bright parts of the surface of the planet.

To the east of them there was a "break" or "gap" in the northern edge of the shading about the pole; and, on looking over the sketches and notes made here since 1869, I find that these spots have always, perhaps, been accompanied by a "gap" in the dusky band in which they lay. Sometimes the spots preceded the "gap" in position; sometimes they followed it; and occasionally some lay on both sides of it.

This double phenomenon was observed here in November and December 1869, in January, October, and November 1870, in January 1872; and now it appears again.

I believe that such bright spots have but rarely been seen to the north of the equator of *Jupiter*.

Mr. Edward Crossley's Observatory,
Skircoat, Halifax,
1874, *April* 24.

Discovery of Minor Planet (137).

This planet was discovered by M. Palisa, at Pola, on April 21st, its position being—

	Mean Time at Pola.	R.A.	N.P.D.
	h m	h m s	° ′
1874 April 21	11 56	13 20 16	98 17

An observation made at Berlin on the following day gave—

	Mean Time at Berlin.	R.A.	N.P.D.
	h m	h m s	° ′
April 22	10 7	13 19 38	98 9·4

The planet was of the 12th magnitude.

Discovery and Elements of Comet II. 1874, *and Comet III.* 1874.

The first of these Comets was discovered by Dr. Winnecke, at Strasburg, on April 11th, its position being—

	Mean Time at Strasburg.	Apparent R.A.	Apparent N.P.D.
	h m	h m s	° ′
1874 April 11	15 30	21 23 8	96 56

From observations made at Kremsmünster and Vienna on

April 12th, at Leipzig on April 17th, and at Vienna on April 20th, the following elements have been calculated by Professor E. Weiss:—

$$T = 1874 \text{ March } 13{\cdot}99342 \text{ Berlin Mean Time.}$$

$$\left.\begin{aligned} \pi &= 245^\circ\ 53'\ 14'' \\ \Omega &= 274\ \ 6\ 44 \\ i &= 148\ 24\ 42 \end{aligned}\right\} \text{Mean Equinox } 1874{\cdot}0$$

$$\log q = 9{\cdot}94743$$

A comparison of the middle observation with the corresponding place calculated from these elements gives—

$$\Delta\ \alpha \cos \delta = -0^s{\cdot}75$$
$$\Delta\ \delta = -2''{\cdot}5$$

The third Comet of 1874 was first seen by M. Coggia, at Marseilles, on April 17th, its observed place on that day being—

	Mean Time at Marseilles. h m s	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″
1874 April 17	9 29 43·9	6 28 7·47	20 2 38·3
17	13 8 42·0	6 27 57·80	20 3 16·6

From the observations made at Marseilles on April 17th, and two Twickenham observations of April 28 and May 9, Mr. Plummer has calculated the following elements:—

T	July 6·38065 G.M.T.	
	° ′ ″	
π	270 20 36·4	App. Eq. May 0
Ω	117 34 13·8	App. Eq. May 0
i	64 29 28·2	
log q	9·8234364	

Motion direct.

The middle observation is thus represented:

$$\text{C}-\text{o,} \quad \text{long.} + 7'{\cdot}6$$
$$\text{lat.} \quad 9{\cdot}0$$

The elements show a similarity with those of Comet II. 1737.

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXXIV. *June* 12, 1874. No. 8.

Professor ADAMS, President, in the Chair.

Wm. E. Mitchell, Esq., The Fort, Norquay, Cornwall;
C. R. Tompkins, Esq., H.M. Dockyard, Portsmouth; and
G. H. With, Esq., Hereford;

were balloted for and duly elected Fellows of the Society.

The Society having Removed from Somerset House, all Letters, Parcels, and Communications are in future to be addressed ROYAL ASTRONOMICAL SOCIETY, BURLINGTON HOUSE, Piccadilly, W.

Note on a Paper by Mr. Christie, "On a Method of connecting the Curvature of the Lines in the Dispersion Spectrum." By William Simms, Esq.

In an investigation into the cause of the curvature of the lines in the Dispersion Spectrum, March No., p. 263, Mr. Christie has shown that a correction may be obtained by a direct reflexion through the same plane; but when the ordinary right-angled prism is used at the end of the train for the purpose of returning the spectrum, the evil is propagated, precisely as it would be had it followed its original course through a similar number of prisms.

Upon reading Mr. Christie's paper, the idea occurred to me, that by employing a prism of three reflexions, the correction would also be obtained, carrying on the spectrum at a higher plane, as with the right-angled prism; and so this improvement could be readily made to existing spectroscopes.

Mr. Christie has suggested, that to avoid the loss of light from the middle portion of the train, the upper and lower halves should be divided, raising the upper halves; this might be desirable in a new instrument, but would be a costly alteration to an old one. I think there would generally be sufficient light without having recourse to this expedient; if thought desirable, a detachable prism might be applied, and this, or the two reflexion prisms, used at pleasure.

Burnham,
1874, *May* 4.

Professor C. V. Zenger, in two letters addressed to Mr. Glaisher, dated Prague, January 17 and March 22, 1874, proposes a method of enlarging lunar and solar photographs so as to correct the spherical aberration, and makes suggestions as to the use of photographs in the coming transit of *Venus*.

In the first instance he gets very sharp images by using a parabolic spectrum bought from Mr. Browning, the rest of the spherical aberration being destroyed by a very fine aplanatic lens from Steinheil with negative focal length. He got in this way solar images with spots on them of, perhaps, the greatest sharpness ever obtained, with faculæ, willow leaves, and a most sharply defined gradation of penumbra, the diameter of the Sun's image being from 20 to 24 inches.

He then conceived that the remainder of aberration got in the images might be easily destroyed by photographing with lenses whose remainder of error was opposite to that of the photographing speculum or refractor, and tried the experiment with much delight, on a fine sample of Rutherford's 15-inch lunar photograph, with a very good, but a little over-corrected, opera-glass from Paris, magnifying 8 to 10 diameters. Not only the lunar photograph got a wholly improved aspect, the influences of unevenness being nearly destroyed, (falling in the opposite direction to the spherical aberration), but objects could be detected in it invisible, or very uncertain, even with the aid of an excellent magnifier by Steinheil. Ridges, shadows, and inclined planes, slopes of the *illuminated* and uneven surface of the inner parts of the craters, not or scarcely visible with the naked eye and magnifying aplanatic lens, became as sharp and definite as he could see them in one of the clearest and driest winter nights with Browning's 5-inch reflector with 100 and 300 magnifying power.

Some lunar photographs originally 3 inches in diameter, and enlarged by Brothers of Manchester to 11 inches, Professor Zenger has in this way enlarged to 110 inches, without losing, and indeed gaining, definition. Specimens (on paper) of *Clavius* and its vicinity accompanied the letters, and were exhibited at the meet-

ing of the Society; but he proposes to bring with him to he ensuing meeting of the British Association glass positives on the scale of 80 to 110 inches.

He concludes with suggestions as to photographing the transit:

1°. It is of no use to photograph the moment of the contact, because accuracy is entirely destroyed by the effect of the interference.

2°. It would be better to have the passage photographed at intervals so as to ascertain exactly the moment at which *Venus* passes through a determined meridian on the Sun's surface. The Sun's apparent diameter being nearly the Moon's or 31′, it can by his process easily be enlarged to 110 or more inches diameter, one inch corresponding to $\frac{31'}{110} = 0'{\cdot}0282$ or $1''{\cdot}7$. By subdividing to one-hundredth, it would be possible to ascertain the position of *Venus* on the Sun's disc up to $0''{\cdot}017$ or $0^s{\cdot}001$ in time, an amount of precision scarcely obtainable by the method proposed by M. Janssen at the last meeting at Bradford. But he conceives the magnifying might even go to 200 or 250 inches.

On a remarkable Structure visible upon the Photographs of the Solar Eclipse of December 12, 1871.

By A. Cowper Ranyard, Esq.

The structure which I am about to describe is by no means a marked feature on the Indian photographs; indeed, it was not observed until after nearly a year had been spent in cataloguing the details which are to be made out on the different negatives. When, however, it has once been pointed out, no careful observer can have any doubt as to its existence; and the tardiness with which it was observed may perhaps be accounted for by the fact that attention was principally directed to an examination of the dark or partially opaque details of the photographs which correspond to the luminous details of the corona, whereas this was a bright or transparent structure; and bright spots, lines, or patches had always been regarded as photographic defects, and consequently but little attention had been paid to them.

The original negatives are very small: the dark moon is represented by a transparent circle about $\frac{3}{10}$ths of an inch in diameter, and the whole extension of the corona could be covered by a sixpence. The separate details of the coronal structure are therefore very minute, and it would be impossible from the examination of a single negative to determine whether any small marking has its origin in some almost microscopic impurity on the collodion, or whether it represents a vast mass of many million cubic miles in

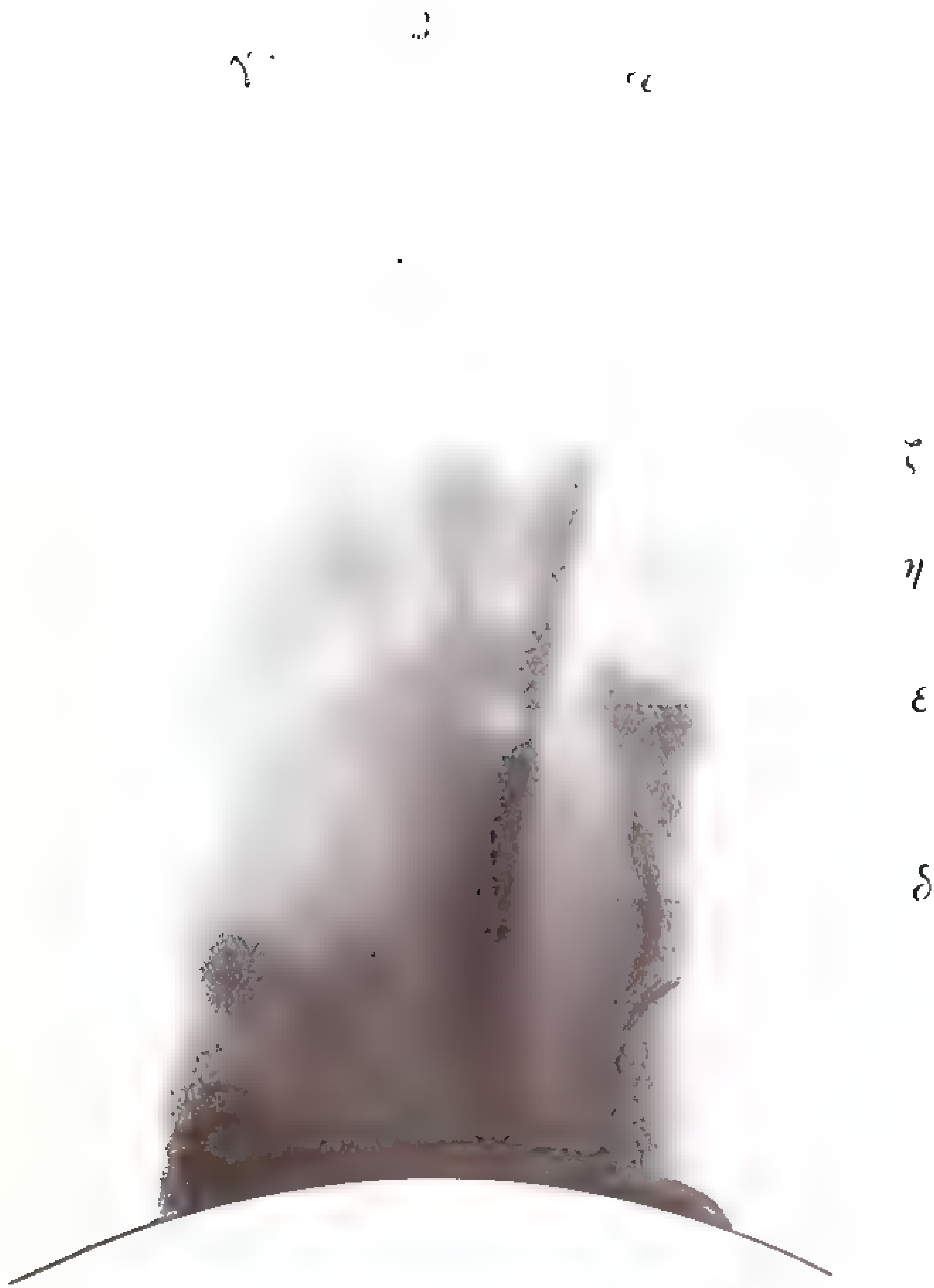
the corona: it is only by a careful comparison of the different negatives that such photographic flaws can be properly eliminated. For this purpose a Catalogue has been made, containing a list of the negatives upon which each detail can be distinguished, and the details entered in the catalogue have also been drawn. In this work I have been fortunate enough to be aided by a most accurate and conscientious artist (Mr. Wesley), for whose laborious perseverance in the task I cannot be too thankful.

All the details cannot be seen on any one negative; in some the structure of the lower parts of the corona is all that can be made out, while in others the middle heights are best seen, the extreme extension of the corona being lost, and the lower parts merged in an opaque mass by reason of over-exposure. Again, on any one negative, all the details cannot be seen at the same time, they only become visible as the plate is examined with different amounts of transmitted light, and with different magnifying powers; for example, the prominence structure which extends to a height of 2′ or 3′ can only be made out with strong transmitted light, and a moderately high magnifying power, whereas the structure of the corona at a height of from 10′ to 15′ can only be seen on the background of a bright sky, and is completely lost when a lens is used. The very furthest extensions of the corona which can in some instances be traced to a distance of quite 26′ from the Sun's limb can only be seen by reflected light. In making the catalogue no details have been entered or drawn which could not be traced on three of the negatives.

While working at a group of coronal structure* on the eastern equatoreal limb, Mr. Wesley noticed that a small bright spot, or flaw as we then considered it, occupied apparently the same position in negatives 1 and 4 of Lord Lindsay's series. On examining the others of Lord Lindsay's or the Baikul series we found that a bright spot or flaw was more or less distinctly to be traced in the same place on all of them.

I at first thought that it must be due to a star seen through the corona, but a little reflection showed us that this explanation could not be sustained, for the image of a star would have been represented by a dark or very opaque point, whereas this was bright: and therefore, the collodion had at this point been less acted upon than by the light from the surrounding details. On a closer examination of the plates upon the next fine day, three partially transparent circular arcs, concentric with the bright spot, were detected above it. The middle one of the three arcs is the most distinct, and can be traced without any doubt upon four out of the five Baikul negatives.

* It should be noted that in the plate appended to this paper the distinctness of the coronal details is intentionally exaggerated. Not only would it have been very difficult to reproduce by lithography the delicate differences of intensity which are traceable in the negatives, but, if it had been possible, the plate would not have so well served the purposes of a diagram.



STRUCTURE VISIBLE UPON THE PHOTOGRAPHS
OF THE ECLIPSE OF DEC. 12TH 1871.

(Black representing opacity in the Negatives.)

This circular and concentric structure is so different from all the other forms traceable among the dark details of the corona, that I was loath to accept it as being in any way connected with the Sun. The idea struck me that it might be due to reflexions taking place within the camera, but such reflexions would always have been symmetrically situated with regard to the camera, and therefore always have fallen on identically the same part of the plate; whereas, although the structure is always to be found on identically the same part of the corona, the image of the corona is shifted upon the different plates. Again, any reflexions of a bright image or ghost would have given rise to a dark structure upon the photographs. There seemed, therefore, no alternative but to suppose that the structure was due to some partially opaque body situated between us and the Sun, cutting out or partially intercepting the light of the corona. At this time I had only Lord Lindsay's negatives and two enlarged copies of the Java photographs, to which I could refer. As all traces of the structure were lost on the copies of Lord Lindsay's negatives, I was not surprised to find that it could not be seen on the Java enlargements. After a short time I obtained the loan of Colonel Tennant's original negatives from Mr. De La Rue. They were taken at Ootacamund, a distance of more than 120 miles from Baikul, and I was therefore not a little astonished and pleased to find that the central bright spot was traceable on five out of the six negatives of his series. The central arc was also just traceable on four out of the six negatives, and the inner arc is to be made out on three of the negatives.

In the catalogue the central bright spot has been lettered E.ϵ. The most distinct of the three arcs is lettered E.ζ., and the other two arcs are taken together under the heading E.η. The descriptions run thus:—

E.ϵ. (Position-angle 85°.) A minute bright spot about 9′ from the Sun's limb. It appears to encroach on the Northern edge of E.β, and to touch the top of E.δ. It is best seen upon the background of a clear sky.

B.1. (*i.e.* negative No. 1. of the Baikul series) distinctly visible.

B.2. Distinctly visible.

B.3. Distinctly visible, though not so clearly marked as in B.1. B.2. and B.4.

B.4. Distinctly visible.

B.5. Just visible.

O.1. (*i.e.* negative No. 1. of the Ootacamund series) visible.

O.2. Distinctly visible.

O.3. Distinctly visible.

O.4. Only just to be made out and not with certainty.

O.5. Only just visible.

O.6. Lost.

E.ζ. A bright arc about $1\frac{1}{2}'$ broad, with a radius of about $6'$, it appears to be concentric with E.ϵ., and is concave towards the Sun. At its highest part it is about $16'$ above the Sun's limb. On the South it cuts across E.γ. E.β. E.α. and on the North extends to a distance of about $4'$ on to the area of D.η.

B.1. Very distinctly visible.
B.2. Quite lost.
B.3. Just visible.
B.4. Very distinctly visible.
B.5. Distinctly seen, though not so clear as in B.1. and B.4.
O.1. Visible, though its central portions are interfered with by a streak (the streak is evidently due to drainage action which took place while the plate was wet).
O.2. Just visible, at first sight it appears to be traceable down as far as the Southern branch of D.ϵ., but possibly this part of the curve is formed by a photographic defect. It gives a distinctly parabolic appearance to the arc.
O.3. Distinctly visible.
O.4. Distinctly visible.
O.5. Lost.
O.6. Lost.

E.η. The traces of a circular arc much fainter than E.ζ. It appears to be concentric with E.ϵ, and has a radius of about $3'$. The rays E.α., E.β., and E.γ. can be distinctly traced through it, and appear almost to break it up into three bright spots. An outer concentric circular arc with a radius of about $10'$ is also just visible, but as it can only just be traced upon three of the negatives, a separate letter has not been given to it in the catalogue.

B.1. Distinctly visible; the outer arc is also to be seen.
B.2. Quite lost.
B.3. Just traceable.
B.4. Distinctly visible; the outer arc is also to be traced.
B.5. Just visible with a good light.
O.1. Distinctly visible with a good light, though it is cut across by an opaque streak.
O.2. Just visible.
O.3. Distinctly visible; the outer arc is also to be traced.
O.4. Lost.
O.5. Lost.

No difference can be detected in the position of the central spot and concentric arcs relatively to the details of the corona in passing from the Baikul to the Ootacamund series; and taking into consideration the distance between the two stations, it is evident that the structure must be either due to some dark body in the corona, or to some semi-transparent body situated between us and the corona, at a great distance from the Earth.

The form of structure is similar to that which has often been

observed in the nuclei and the concentric comæ of comets; and it seems not very unreasonable to suppose that this may really be a photograph of a faint though large comet near to perihelion.

On this supposition, it is not perhaps very remarkable that the structure should not have been detected by the many observers who were engaged in examining the corona during totality—for if the comet was similar in chemical composition to those whose spectra have been observed by Mr. Huggins and Professor Young, the loss of light caused by its absorption would have been more readily detected on the photographs than by the human eye, for the two most refrangible bands of the carbon comets lie in parts of the spectrum which produce but a feeble effect upon the retina, though the actinic action is there very strong. The two series of photographs are not sufficiently removed from one another in point of time to show any motion of the comet over the details of the corona. The date of Colonel Tennant's No. 1 negative is about one minute of time after Lord Lindsay's No. 1; and Colonel Tennant's No. 5 is about three minutes after Lord Lindsay's No. 1; but we have been unable to trace any difference in the position of the nucleus in passing from Lord Lindsay's No. 1 to Colonel Tennant's No. 5.

It is worthy of remark that a somewhat similar structure was observed by Professor Winnecke during the Eclipse of 1860. A drawing of it has been published in the *Mémoires de l'Academie Impériale de St. Pétersbourg*, VII^e Série, Tome IV., No. 1. It consisted of a single parabolic arc, with its convexity turned towards the Sun, and appeared as if drawn with sepia upon the bright background of the corona. Dr. Winnecke describes it, at page 39 of the above Memoir, as "einen parabolisch gekrümmten, dunklen Bogen im Lichte der Corona. . . . Dieser Bogen schien mir gleichsam mit Sepia auf dem lichten Grund der Corona gezeichnet zu sein."

An undoubted comet has been seen projected on the brilliant background of the photosphere (I refer to Pastorff's observations of the Comet of 1819*). And much fainter comets would be visible when projected on the comparatively feeble light of the corona. It seems, therefore, not very improbable that both this structure and the parabolic arc observed by Dr. Winnecke may have been due to comets which happened to be situated between us and the corona during times of total eclipse. And it is not impossible that such giant comets may exist in great numbers in the immediate neighbourhood of the Sun, though by reason of their faintness, or the short time of their ebullition, they are not visible to us either before or after their perihelion passage.

* As well as to the nebulous spot observed by him upon the Sun in May 1828. See the *Monthly Notices*, November 1873.

Notes in reply to Sir John Herschel's "Queries relative to Double Stars." By S. W. Burnham, Esq.

The following Queries as left by the late Sir John Herschel were printed in the *Monthly Notices* for November 1871, by the Committee appointed by the Royal Astronomical Society, to examine and report upon a collection of papers bequeathed by him to the Society for the preparation of a "General History of Double Stars," with the request that observers should aid in removing the doubts existing in reference to these stars. As stated in the recent Report of the Council, one of the forthcoming volumes of *Memoirs* will contain this very valuable and needed contribution. I was not aware until within a few months of these Notes of Herschel; but some time previously prepared a list of several hundred double stars requiring telescopic examination, and which included most of the following objects. The list might be very much extended. I select from a large mass of notes such as pertain to the double stars in question. The observations have been made exclusively with a 6-in. Clark refractor.

h 2021 $\left\{\begin{array}{l} \text{R.A.} = 1^{h}\ 0^{m}\ 37^{s} \\ \text{P.D.} = 109^{\circ}\ 31'\ 40'' \end{array}\right\}$ Is it really close double?—"Violent suspicion.

Examined on only occasion only, but no double star found in this place. The night, however, was not first class, and if a *very* difficult object, it might not have been seen.

Σ 168 $\left\{\begin{array}{l} \text{R.A.} = 1^{h}\ 40^{m}\ 17^{s} \\ \text{P.D.} = 20^{\circ}\ 47'\ 30'' \end{array}\right\}$ Not seen double by h.

I have just noticed for the first time, that the above P.D. as given by Herschel is incorrect. It should be, 24° 7′ 31″. I have looked for this pair several times, but I think on each occasion used Herschel's P.D., and of course failed to find it. Herschel's observation occurs in his Fourth Catalogue, and there the place is correctly stated, so that it was properly searched for by him. It is now too low to do anything with.

Σ 211 $\left\{\begin{array}{l} \text{R.A.} = 1^{h}\ 55^{m}\ 54^{s} \\ \text{P.D.} = 96^{\circ}\ 14'\ 50'' \end{array}\right\}$ No such star found in or near place by h.

I could not find any star as bright as 8th mag. in this place: there are some small stars, but none of them were seen double. This is one of Struve's rejected pairs, and is not included in *Positiones Mediæ*, or *Mensuræ Micrometricæ*. It is given in Struve's early Catalogue as Class IV., and magnitudes (8), (10).

Σ 343	R.A. = $2^h\ 55^m\ 23^s$ P.D. = $6°\ 35'\ 32''$	Are these the same double star? Σ is confused, and *h* disagrees.
Σ 347	R.A. = $2^h\ 56^m\ 27^s$ P.D. = $6°\ 35'\ 2''$	

In *Mensuræ Micrometricæ* it is stated that the two are identical, there being an error of 1^m in the R.A. of the second. Dembowski's measures of this pair show a slight change in the distance of the components, the angle remaining the same. Struve found, P = 325°·4; D = 22″·66, (1832·6). Dembowski gives, P = 325°·2; D = 24″·95 (1865·0). I am not acquainted with any other measures.

Σ 407	R.A. = $3^h\ 21^m\ 56^s$ P.D. = $101°\ 43'\ 4''$	Is the minute of R.A. 19 or 21? Σ's two catalogues differ.

The place of this pair appears to be the same ($3^h\ 21^m·8$) in both Struve's Catalogues. It is not in *Positiones Mediæ*. I found it, readily identified it as L 6490, the place of which for 1830 is:

R.A. = $3^h\ 21^m\ 52^s$ N.P.D. = $101°\ 43'\ 40''$

Σ 417 Σ 518	R.A. = $4^h\ 7^m\ 27^s$ / $4^h\ 7^m\ 48^s$ P.D. = $97°\ 55''\ 22''$ / $97\ 54\ 23$	40 *Eridani.* Confusion exists. Wanted an exact diagram, description, and measures of A, B, C; and ? of a fourth star D.

Struve's place for 1830 (*Pos. Med.*) is:

R.A. = $4^h\ 7^m\ 27^s$ P.D. = $97°\ 55'\ 22''$

This agrees exactly with the British Association Catalogue. There are four companions, the measures of which are as follow:—

	Pos. °	Dist. ″	Mags.	
A and B	107·2	83·48	4, 9·1	Struve (1836·0)
B „ C	159·9	3·±	10½, 11	Dawes (1851)
A „ D	185·0	75·85	...	Winnecke (1864·8)
A „ E	312·5	89·45	...	Winnecke (1864·8)

Engelmann for A and B (1863·1) gives 105°·4 : 82″·83. The close pair, constituting a binary system in rapid orbital motion, was found by Winnecke (1864·8) to give P = 147°·6 ; D = 4″·45.

Knott (1871) estimates the angle and distance 125° ± : 2″ ±. My 6-inch refractor shows readily all these stars, except perhaps E, which was not particularly looked for. Struve speaks of a 12 mag. star at 16″·79 in the direction of 96°·8. I do not find any later or other mention of this star.

		h	m	s	
Σ 609	R.A. =	4	41	59	? whether Struve's place, as here given from *Pos. Med.*, or that of my *Seventh Catalogue*, $4^h\ 43^m\ 4^s$, 89° 3′ 17″, be right. Positions and distances agree. Or are they really two different double stars? *My place* is corroborated in *Fifth Catalogue*, *h* 2240 *bis*, making the R.A. 43^m.
		°	′	″	
	P.D. =	89	8	59	

I examined this for the purpose of seeing if it was identical with *h* 686, given by Herschel without measures or magnitudes, with this note: "Close double; taken for Σ 609, but clouds prevented its description." I looked for it on a very superior night, but could not, or did not see it. Subsequently I found it very readily, and noted it as "very easy," estimating independently the distance at 2″, and the angle 80°. Struve gives D = 1″·93; P = 82°·1. There is something rather strange about this pair, unless by some oversight Dembowski missed it. He has this remark in his measures of double stars made in 1865·9: "Simple; bonnes conditions." He also found it single, 1866·1. Struve's mags. are 8·5, 8·7, which should make it, as I found it, an easy pair. Without ascertaining positively, it appears to be L 9101, the place of which agrees exactly with *Pos. Med.* I could not find any other pair in the neighbourhood, and have no doubt *h* 686 is the same as Struve's pair. Herschel's declination of that star agrees with Struve.

		h	m	s	
Σ 662	R.A. =	5	5	39	In *Pos. Med.* the P.D. is 94° 15′ 54″, but in the *Dorpat Catalogue*, and also in the *Mens. Mic.*, the degree of P.D. is 64°. The declinations given are: *Dorpat*, +25° 45′; *Mens. Mic.*, +25° 45′; *Pos. Med.*, −4° 15′ 54″.
		°	′	″	
	P.D. =	94	15	54	
		64	15	54	

The place in *Mens. Mic.* is correct. The companion, although rated by Struve as 11th magnitude of his scale, was well seen with a power of 400, and is one of those minute stars which are improved by magnifying. There is no double in the place set down in *Pos. Med.* The star in question is L 9809.

		h	m	s	
h 5465	R.A. =	5	39	18	An excessively minute *comes* 12″ distant suspected. ? if verified.
		°	′	″	
	P.D. =	78	4	3	

I have looked for this several times, once when the conditions were remarkably favourable, but have never been able to see the least trace of a companion. If it exists at all, which is very doubtful, it must be an object of excessive difficulty.

$$\Sigma\ 923\left\{\begin{array}{l} \text{R.A.}=\left\{\begin{array}{lll} 6^{h} & 22^{m} & 18^{s} \\ 6 & 42 & 15 \end{array}\right. \\ \text{P.D.}=30^{\circ}\ 25'\ 8'' \end{array}\right\}$$

? which is the right minute of R.A. Σ leaves it doubtful, and I have no observation of it.

This is one of Struve's rejected pairs, described in *Catalogus Novus* as Class IV. (6), (10). The correct minute of R.A. is 21. I am not aware of any measures having ever been made of this pair. The companion is too distant to make it a very interesting object, and for this reason it was not included in *Mens. Mic.* The companion is in the direction of about 140°. There is a more distant companion *s* a little *p*.

$$\Sigma\,1006\left\{\begin{array}{l} \text{R.A.}=6^{h}\ 51^{m}\ 22^{s} \\ \text{P.D.}=\left\{\begin{array}{lll} 26^{\circ} & 59' & 18'' \\ 27 & 10 & 18 \end{array}\right. \end{array}\right.$$

Which is the right P.D.? In the *Dorpat Catalogue* the declination is +63° 1′; in the *Mens. Mic.*, +62° 50′. Which is right?

This is a very wide pair (30″·59) of moderately bright stars, both of which are given in Argelander's *Zones* = *Arg.* (+ 62°) 902, 904. The place for 1855 of the principal star is:

$$\text{R.A.}=6^{h}\ 53^{m}\ 27^{s} \qquad \text{P.D.}=27^{\circ}\ 14'\!\cdot\!6$$

This does not agree exactly with either of Struve's Catalogues. I have not seen any measures of this pair made since Struve's observations.

$$\begin{array}{l}\Sigma'\ (989)\ g.\ c. \\ \sigma\ 295\end{array}\left\{\begin{array}{l} \text{R.A.}=\left\{\begin{array}{lll} 8^{h} & 14^{m} & 22^{s} \\ 8 & 14 & 25 \end{array}\right. \\ \text{P.D.}=\left\{\begin{array}{lll} 92^{\circ} & 25' & 7'' \\ 92 & 23 & 51 \end{array}\right. \end{array}\right\}$$

Set down as "very near" σ 295 (*Catalogi Prioris*). Are these really two different double stars? If not, which is right?

The first refers to the general number in *Positiones Mediæ*. There is but one pair in this place, and that is a very wide double, the companion nearly preceding, and about the 10th mag. The star is L 16441, and the place for 1830 from that Catalogue is:

$$\text{R.A.}=8^{h}\ 14^{m}\ 21^{s} \qquad \text{P.D.}=92^{\circ}\ 25'\ 0'',$$

agreeing with *Positiones Mediæ*. It is an uninteresting object, and so far as I am aware, has never been measured or included in any modern catalogue. I have carefully examined all the stars in the vicinity, and fail to find any other pair nearer than Σ 1233.

$$\Sigma\ 1259\left\{\begin{array}{l} \text{R.A.}=8^{h}\ 32^{m}\ 7^{s} \\ \text{P.D.}=51^{\circ}\ 5'\ 29'' \end{array}\right\}$$

In *Dorpat Catalogue* 8ʰ 31ᵐ·9, decl. +38° 56′ in *Mens. Mic.* 8ʰ 36ᵐ, decl. +39° 5′. Query, Which is right? *h* has no observation.

The place given is from *Positiones Mediæ*, which differs from Struve's other Catalogues both in R.A. and P.D.; and strangely enough they are all in error, but that from *Mensuræ Micrometricæ* is the nearest and substantially correct. Σ 1259 is identical with Weisse VIII. 937, and its place for 1830 from that Catalogue is:

$$\text{R.A.} = 8^{h}\ 35^{m}\ 35^{s} \qquad \text{Decl.} = +39^{\circ}\ 6'\ 3''.$$

I did not find any star corresponding to the place in *Positiones Mediæ*—at least no star as bright as the 9th magnitude, while that star is rated by Struve as 7·5. Either the star is variable, or there is some error in the observation, probably the latter. Upon the first examination of this vicinity, I found a very pretty pair, which I then took to be Σ 1259, with a very considerable change both in angle and distance. For his pair Struve gives, P=4″·97; D=340°·9. The distance of the pair found was certainly not greater than 2″, and the angle obviously more than 340°. A mean of several measures, differing but slightly from each other, of the angle gave 355°·5. This star is Weisse VIII. 849, and the place for 1830:

$$\text{R.A.} = 8^{h}\ 35^{m}\ 5^{s} \qquad \text{Decl.} = +39^{\circ}\ 24'\ 35''.$$

Finding that it differed from *all* the places given of Σ 1259, I made a further search, and at once found Struve's pair in the place given above. On the first occasion I examined all the stars that were prominent in the finder, but this escaped attention from its faintness. It is hardly possible that Struve should have overlooked the new pair, unless some change has occurred since his observations. It is nearly one magnitude brighter than Σ 1259; and as the components are about equal, it makes a very easy object, and one not likely to be overlooked with any aperture above 3 inches.

δ Cancri $\left\{\begin{array}{l}\text{R.A.} = 8^{h}\ 35^{m}\ 1^{s}\\ \text{P.D.} = 71^{\circ}\ 13'\ 39''\end{array}\right\}$ *h* 457 makes position 160°, distance 25″. Lassell, *Astr. Nach.*, No. 858, states it as, position 121° 12′, distance 45″. What is it really?

The companion though rated by Herschel as 15 magnitude, was noted by me as "well seen with 400; the distance certainly more than 25″—probably not less than 45″. The angle appears to be nearer 120°, but too minute to estimate with great certainty." Subsequently I made three measures of the angle, the mean of which gave P=120°. The companion is very faint, but the separate results are sufficiently accordant (120°·5, 118°·5, 121°·5) to make it apparent that Herschel erred in his estimate of the angle. I made no attempt to measure the distance; but it is obviously greater than the value assigned by Herschel, and undoubtedly Lassell's result is also correct in that respect.

h 2616 $\left\{\begin{array}{l}\text{R.A.} = 12^{h}\ 29^{m} \\ \text{P.D.} = 75^{\circ}\ 17'\end{array}\right\}$ Close to a star of 9th mag. is a small, well-defined body, *which may be* a close double star. *np* is a very faint nebula (no doubt III. 602, or Neb. No. 3113 of my *General Catalogue*). Is it there still? Was it a planet?

In Herschel's "Queries" the name or number of the star is erroneously printed *δ Cancri*. It should be as I have given it, *h* 2616. This is a faint double star, and readily seen with a 6-in. aperture when the conditions are favorable. It is strange that Herschel should have had any doubt about it, unless his instrument was not properly in focus at the time, or the definition was very poor, when a close, faint pair might present some such appearance as that described. I estimated the angle of this pair as about 310°, and distance perhaps a little more than 2″. Their 9th magnitude star near it is L 23620, given in Lalande as 8th magnitude (and by another observation 7) the place of which (1830) is:

$$\text{R.A.} = 12^{h}\ 30^{m}\ 2^{s} \qquad \text{P.D.} = 75^{\circ}\ 15'\ 15''.$$

I measured the distance and angle of the faint pair from this star, obtaining P=237°; D=93″. The very faint nebula referred to, I did not notice.

κ Virginis $\left\{\begin{array}{l}\text{R.A.} = 14^{h}\ 3^{m}\ 50^{s} \\ \text{P.D.} = 99^{\circ}\ 28'\ 42''\end{array}\right\}$ *σ* 453 (*Catalogi Prioris*). Is it really a double star?

It is not properly speaking a double star. There are two distant companions, one nearly following, the other preceding, both too far away to make it an object of any interest. No measures have ever been made, nor is it found in any catalogue where measures are given.

ω Boötis $\left\{\begin{array}{l}\text{R.A.} = 14^{h}\ 54^{m}\ 40^{s} \\ \text{P.D.} = 64^{\circ}\ 18'\ 58''\end{array}\right\}$ *σ* 469. Is it really double?

There are some minute stars in the field, but they are too distant to be regarded as companions. Examined on a superb night; the star perfectly round, with 400. This is not included in any regular or original catalogue of double stars.

h 1271 $\left\{\begin{array}{l}\text{R.A.} = 15^{h}\ 15^{m}\ 18^{s} \\ \text{P.D.} = 107^{\circ}\ 59'\ 48''\end{array}\right.$

h 4768 $\left\{\begin{array}{l}\text{R.A.} = 15^{h}\ 15^{m}\ 18^{s} \\ \text{P.D.} = 109^{\circ}\ 0'\ 43''\end{array}\right.$

Are these identical? and, if so, which P.D. (differing 1°) is the right? The measures agree.

In the P.D. of the latter (from *Cape Observations*) there is undoubtedly an error of 1°. The measures and magnitudes ot the two pairs correspond very closely. I found *h* 1271 at once in the place assigned, but could find no double star at all in the neighbourhood of 1° further south.

$$h\ 256\left\{\begin{array}{llll} \text{R.A.} = & 15^{h} & 32^{m} & 2^{s} \\ \text{P.D.} = & 71^{\circ} & 40' & 0'' \end{array}\right\}$$ Not found subsequently; but ? if properly looked for. Does it exist? Measures 5° *sf*; distance 2″.

I have not been able to find any such pair in this place. I have also examined the catalogues to see if there is any which might have been observed and the degree of P.D., or hour of R.A. erroneously noted, but do not find any pair likely to be the one in question. Herschel has not given the magnitudes of this pair.

$$\Sigma\ 2012\left\{\begin{array}{llll} \text{R.A.} = & 16^{h} & 0^{m} & 49^{s} \\ \text{P.D.} = & 97^{\circ} & 48' & 13'' \end{array}\right\}$$ Is this really identical with B.A.C. 5379?

This is one of Struve's rejected stars, and of course given in the early catalogue without measures. This is not B.A.C. 5379, but is L 29435, which is 48ˢ *p* and 2′ 52″ *n*. My estimates of angle and distance were: P=270°; D=15″; mags. 8, 13. I have since found an observation of this pair in *Cape Observations*, where P=256°·7; D=20″±; mags. 8½, 11. I know of no other measures. Herschel gives the place in that work for 1830:

R.A. = $16^{h}\ 0^{m}\ 49^{s}$ P.D. = $97^{\circ}\ 47'\ 27''$

The place (substantially the same) from Lalande for the same epoch is:

R.A. = $16^{h}\ 0^{m}\ 50^{s}$ P.D. = $97^{\circ}\ 47'\ 54''$

$$h\ 584\left\{\begin{array}{llll} \text{R.A.} = & 16^{h} & \left\{\begin{array}{c}7\\13\end{array}\right\}^{m} & 58^{s} \\ \text{P.D.} = & 50^{\circ} & 20' & 45'' \end{array}\right\}$$ R.A. between 7^{m} and 13^{m}. ? the right minute.

The seconds of R.A. should be 48^{s} to correspond with the original observation in the second Catalogue of Herschel, and the R.A. is there stated to be between $16^{h}\ 7^{m}$ and $16^{h}\ 12^{m}$. I find that the correct minute is 12. This is a faint and insignificant pair.

$$\left.\begin{array}{l} \Sigma'\ (1814)\left\{\begin{array}{llll} \text{R.A.} = & 16^{h} & 16^{m} & 48^{s} \\ \text{P.D.} = & 88^{\circ} & 23' & 2'' \end{array}\right. \\ \Sigma\ 2041\left\{\begin{array}{llll} \text{R.A.} = & 16^{h} & 16^{m} & 48^{s} \\ \text{P.D.} = & 88^{\circ} & 21' & 35'' \end{array}\right. \end{array}\right\}$$ "Loco 2041." Are these really two double stars? and, if so, to which does the description, position 4°·4, distance 3″·06, belong?

The first number refers to *Positiones Mediæ*, and the place is from that Catalogue. The second place is the same as that in *Mensuræ Micrometricæ*. It is somewhat remarkable that the fact should have been, so far as I am aware, overlooked heretofore that OΣ 308 and Σ 2041 are really the same star. It is a very beautiful pair, and has been measured several times since its discovery. Struve (1831·4) gave P=4°·4; D=3″·06; mags. 7·3, 10·5. Mädler (1845·5), measuring it as OΣ308, found P=5°·6; D=2″·82, and Dembowski (1865·8) in his measures of the stars composing Otto Struve's Catalogue gives P=1°·9; D=2″·61; mags. 7·2, 9·8. From these results it would seem that the distance must have decreased, while the angle is unchanged. It is worthy of further attention. I have examined very carefully all the other stars in the vicinity, and find only this pair.

Otto Struve's place for 1850 is:

R.A. = 16^h 14^m 14^s P.D. = 88° 26′

I made no special observations for the purpose of identifying this star in the catalogues, beyond ascertaining it was about 12′ north of σ (50) *Serpentis*. There appears to be confusion in reference to the place. None of the stars in Lalande agree with the foregoing. The nearest are (1850):

		h	m	s		°	′	″
L 29860 (8½ mag.)	R.A. =	16	16	44	P.D. =	88	24	1
L 29868 (8 mag.)	R.A. =	16	16	58	P.D. =	88	29	31

I have just referred to Argelander's Catalogue and find a star which agrees with Otto Struve, and is undoubtedly the double in question. It is 11′·1 north of 50 *Serpentis* and 17^s preceding. Its place for 1850 is:

Arg. (+1°) 3112 R.A. = 16^h 14^m 13^s P.D. = 88° 26′·2

Argelander gives the magnitude as 8·2, but it seemed to me rather brighter. It is not in Weisse or Lalande.

		h	m	s	
Mayer	R.A. =	17	1	29	Is there any double star in this place?
		°	′	″	
	P.D. =	114	2	43	

This is 39 *Ophiuchi* (=H III.25=H VI.54=Sh.245). The place does correspond exactly to that given above, but there is no other pair near this place.

		h	m	
Σ 2190	R.A. =	17	29 ±	The measures of this star are very conflicting. Σ (1829) makes position 33°·22 *h* (1830), by a sweep 18°·4 (plainly written in original, and diagram agreeing) Mädler (1843) 33°·67, and Dembowski (1863) 23°·00. Which is right?
		°	′	
	P.D. =	68	53	

I have not examined this sufficiently to account for the discrepancy positively; but it seems probable that Herschel and Dembowski measured, not Σ 2190, but *h* 2807, the R.A. of which is exactly the same, but the Decl. is 25′ less. Struve's measures of his pair are P=33°·2; D=10″·17; mags. 6, 9·5. Herschel for *h* 2807 gives P=22°·4; D=8″±; mags. 7, 11. As will be seen, the two doubles are very similar in magnitudes and distance, with about the difference in angle indicated by the measures given above. I examined this vicinity once, and perhaps twice, and each time failed to see Herschel's pair, notwithstanding the conditions were at least moderately favourable. I am unable to account for the failure to find it, as it should be an easy object, if Herschel's magnitudes are correct. He describes it as "very fine."

H I. 60	R.A. =	18^h	57^m	0^s	Query, as to its existence in this place, or very near it.
	P.D. =	58°	32′	41″	

The name of this star is printed in the "Queries" H 4599, but that is evidently an error. It might have been written H *h* 599 as is No. 599 of Sir John Herschel's Catalogue of his father's double stars. The proper designation of the star is as given above. Herschel states in the catalogue last referred to, that this pair may possibly be Σ 2441 with an error of 5° in the angle. I have shown in a previous paper concerning Sir William Herschel's double stars (*Monthly Notices*, January 1874), that there is very little if any doubt that these two stars are identical. Herschel II. gave P=286°·8; Struve, P=291°·9. I have carefully examined all the stars in this vicinity, and fail to find any other of Class I; *h* 1366 has the same decl. but is about 3^m preceding. The distance (10″·±) is too great for a double of the class described, and both components are too faint to render it of any special interest. In addition to that, the angle is entirely different (57°·8). I noticed an 8th magnitude star about 2½^m *p.* this, with a very minute close double companion, but too faint to be worth cataloguing.

Σ 2545	R.A. =	19^h / 19	27^m / 29	32^s / 33	Smyth described this as "closely following" a star R.A. 19^h 27^m 28^s, and the R.A. 19^h 27^m 32^s given by him differs 2^m from Σ. Which is right?
	P.D. =	100°	31′	57″	

The larger of the two R.A's. is right, and is the place given in *Positiones Mediæ.* This is L 37207, giving exactly the same result. Σ 2541, Σ 2545 and Σ 2547 are in the same neighbourhood, and in some way it is probable Smyth's observations were confused. The identity of this pair with H I. 13, has been clearly shown by Dawes (*Memoirs of the R.A.S.* vol. xxxv), and other evidence bearing upon the same questions has been adduced by Hunt (*Monthly Notices*, January 1872).

		h / °	m / ′	s / ″	
Σ (2360)	R.A. =	19h	40m	4s	Are these not identical; the P.D. applied to H *h* 640 being 11′ wrong?
	P.D. =	57°	31′	25″	
H *h* 640	R.A. =	19h	39m	54s	
	P.D. =	57°	19′	48″	

I have previously called attention to the error in Sir John Herschel's place of this pair (=HN. 110.) This is identical with S. 726. The place is correctly given in *Positiones Mediæ*, as quoted above.

		h / °	m / ′	s / ″	
Σ 2634	R.A. =	20h	1m	50s	Smyth identifies his No. 733 (*Cycle*) with this star, and with H *h* 668; but his P.D. 73° 33′ 54″ (for 1830) differs 8′. Are these two double stars?
	P.D. =	73°	41′	46″	

There is no other pair in or very near this place. This pair (=H II. 70=S. 734) is Weisse XX. 42, 43, corresponding exactly with *Positiones Mediæ*. Smyth's P.D. is erroneous.

		h / °	m / ′	s / ″	
σ 680	R.A. =	20h	22m	53s	Are not the same double stars? Σ 2690 is identified with Dawes (1); *h* (269); H *h* 691; and OΣ 407.
	P.D. =	79°	23′	3″	
Σ 2690	R.A. =	20h	23m	6s	
	P.D. =	79°	18′	22″	

I cannot find any other double star in or near this. This pair is very much confused in the early catalogues. The corresponding numbers are:—H III. 16=Sh. 325=S. 751=σ 681 =OΣ 407=Dawes 1. It is given with measures of the wide pair in South, and Herschel's Catalogue, and again in South's with a different designation, and obviously regarded as a different star. Sir John Herschel included it in his First Catalogue, giving the magnitudes as 9=9, stating "possibly σ 680, but disagreeing much in R.A. and P.D." In a later work he says it is identical with Σ 2690. There are two pairs, in Struve's First Catalogue, σ 680 and σ 681, having the same R.A. with a difference of 10′ in decl. The magnitude of the first is given 8, and that agrees in place with L 39521, and is undoubtedly that star. The second (681) is called H III. 16, and the magnitude and decl. correspond with that pair. There is no companion to L 39521—at least near enough to constitute a double star. H III. 16 is erroneously stated by Otto Struve to be *h* 1521.

		h / °	m / ′	s / ″	
h 2994	R.A. =	20h	36m	18s	17 *Capricorni*. "Requires verification." Position 338°·7; distance 20″±. A *very* minute companion.
	P.D. =	112°	7′	33″	

I have examined this star several times when the conditions were sufficiently favorable to show the companion to α^2 *Capricorni* steadily, and I have never been able to see the least trace of the suspected companion. Herschel entered the magnitude as 20; but I have seen many of his minute companions theoretically far beyond the reach of a 6-inch aperture. I am certain this star, if it exist at all, is not within the grasp of my instrument under any circumstances.

h 1619, *h* 1620	R.A. =	21^{h}	4^{m}	22^{s}	Are not these the same, but 1° P.D. mistakes in one or the other? Which is right? *h* 1620 is described as quadruple.
	P.D. =	76°	9′	37″	
		77	9	36	

I examined both places, and failed to find a double in one of them; but from some confusion in the notes, I am unable to state positively in what star the error occurs.

h 5517	R.A. =	21^{h}	14^{m}	54^{s}	18 *Aquarii.* Query, a very minute companion at 13″ distance.
	P.D. =	103°	36′	6″	

I have never seen the companion, though carefully looked for on several first-class nights. I have found in this general quarter of the heavens a good many new doubles, some of them as difficult as any double stars I have ever seen, and when these were well seen, no companion could be even suspected to 18 *Aquarii.*

Σ 2792	R.A. =	21^{h}	15^{m}	42^{s}	Query, If not the same, with 1° error in P.D. in one or the other; and if so, which is right?
	P.D. =	61°	46′	6″	
h 1638	R.A. =	21^{h}	15^{m}	43^{s}	
	P.D. =	62°	46′	43″	

There is an error in Herschel's P.D. beyond doubt. A careful examination of this region leaves but little uncertainty. On this occasion I found a second minute companion to Σ 2792, not noticed by Struve. The distance is rather more than double Struve's star, or about 20″, and the angle nearly 180°.

Σ 2827	R.A. =	21^{h}	38^{m}	54^{s}	The latter star seems to be only the former with its place better determined. Σ 2827 is a really existing double star = *h* 1690 = Σ 2827 of the *Mens. Mic.* Are these really two double stars very near each other? *h* 1690's place is $21^{h}\ 39^{m}\ 32^{s}$, 27° 11′ 1″.
	P.D. =	27°	9′	54″	
Σ (2628) "Loco Σ 2827"	R.A. =	21^{h}	39^{m}	5^{s}	
	P.D. =	27°	8′	39″	

In the "Query" the P.D. of each star is erroneously printed 28°. It should be 27°. I do not find but the one pair in this place. Herschel's place agrees with *Mensuræ Micrometricæ.* I did not observe this pair sufficiently to identify it in the catalogues.

h 1761 $\left\{\begin{array}{l} \text{R.A.} = 22^{h}\ 16^{m}\ 24^{s} \\ \text{P.D.} = 16^{\circ}\ 0'\ 38'' \end{array}\right\}$ Oblong. Requires re-examination.

Herschel gives P = 40° ± ; D = 1″½ ± ; mags. 12 = 12. I have looked very carefully for this, and in so doing found a new pair (No. 175 of my Third Catalogue, *Monthly Notices*, December 1873), which at first I thought might be Herschel's, but that is not probable, as there is a difference of 12^{m} in R.A. and 9′ in Decl. between the two. Of the new pair I estimated, P = 120°; D = 1″·5; mags. 9½, 9½. It is an exceedingly difficult object, and could only be well seen under very favourable conditions. This was observed several times, and Herschel's pair also looked for, but without success.

h 5529 $\left\{\begin{array}{l} \text{R.A.} = 22^{h}\ 28^{m}\ 57^{s} \\ \text{P.D.} = 95^{\circ}\ 6'\ 4'' \end{array}\right\}$ *κ Aquarii.* An excessively minute companion strongly suspected.

This star, like 17 *Capricorni* and 13 *Aquarii* has defied all efforts to detect the companion.

h 3158 $\left\{\begin{array}{l} \text{R.A.} = 22^{h}\ 53^{m}\ 15^{s} \\ \text{P.D.} = 20^{\circ}\ 8'\ 37'' \end{array}\right\}$ Excessively close. Requires verification. Position 45°.

Carefully examined on a good night, but without success. If there is any double here, which I very much doubt, it must be one of extraordinary difficulty.

Sh. 358 $\left\{\begin{array}{l} \text{R.A.} = 23^{h}\ 46^{m}\ 20^{s} \\ \text{P.D.} = 59^{\circ}\ 6'\ 20' \end{array}\right.$

σ 790 $\left\{\begin{array}{l} \text{R.A.} = 23^{h}\ 49^{m}\ 22^{s} \\ \text{P.D.} = 59^{\circ}\ 11'\ 40'' \end{array}\right.$

? If two distinct double stars. If only one, ? its place.

The second star is called by Herschel *σ* 690; it should be as given here, *σ* 790. There is only one pair in this place, and that is Sh. 358: P = 329°·2; D = 41″·29. It is a very uninteresting pair, the stars small and widely separated. This is L 46844-5, from which its place (1830) is:

R.A. = $23^{h}\ 45^{m}\ 43^{s}$ P.D. = 59° 2′ 37″.

Chicago, 1874, *April* 24.

A Fourth Catalogue of 47 New Double Stars, discovered with a 6-inch Alvan Clark Refractor. By S. W. Burnham, Esq.

The double stars forming the following list have been discovered since the observations embraced in the Third Catalogue which appeared in *Monthly Notices* for December 1873. During a considerable portion of this interval the weather has been exceedingly unfavourable, and the number of new objects found is much smaller than it should have been, judging from former results, with a larger and the usual proportion of clear nights.

These stars have all been found with the same instrument previously used, with the single exception of *τ Orionis*, the duplicity of which was discovered with the 18½-inch refractor of the Dearborn Observatory.

I have measured most of the position-angles; when estimates are made only, the angles have (±) attached to them. In the absence of a suitable driving clock nothing could be done in measuring distances, and the angular measures have been obtained with more difficulty, and doubtless with less accurate results, in consequence of the want of driving machinery. Many of them, however, are very close, or very unequal and difficult of measurement, even with every advantage.

In the following list the distances of thirty are not more than 2″, and many of that number are down to 1″ and less. Several pairs of Struve, Herschel, and others have been found to be again closely double, and some of them are very difficult and interesting objects.

In my last Catalogue, No. 113 was entered as not having been well seen, although there was very little doubt of its being double. Not long since I found this again, independently, and readily recognised it as a double star, but it is an extremely difficult pair.

The places are reduced to 1880. The numbers attached are continued from my former Catalogues in *Monthly Notices* for March, May, and December, 1873.

No. 183 = L 3487.

R.A. = $1^h\ 47^m\ 21^s$ Decl. = $-17°\ 20'$

P = 240 ± D = 2″ Mags. 8½, 11.

A pretty pair in *Cetus*, *n f a*, 6th mag. star. Position-angle not measured. Discovered December 9, 1873.

No. 184 = L 8474.

R.A. = $4^h\ 22^m\ 34^s$ Decl. = $-21°\ 46'$

P = 270°·5 D = 1″·2 Mags. 7, 8.

A very elegant pair in *Eridanus*, and not very difficult under favourable conditions. The angle was measured when the star

was not well seen, and is, therefore, not very accurate. December 14, 1873.

No. 185 = L 8745–6.

R.A. = $4^h\ 31^m\ 25^s$ Decl. = $-\ 15°\ 10'$

P = 241° D = 3″ Mags. 8, 11.

In *Eridanus*, 38′ *s* of 53 *Eridani*, and $1^m\ 17^s$ *f*. Easy and rather unimportant pair. February 17, 1874.

No. 186 = L 8986.

R.A. = $4^h\ 40^m\ 10^s$ Decl. = $-\ 7°\ 12'$

P = 180° ± D = 1″·7 Mags. 8, 10.

A more difficult and much finer pair in the same constellation. No opportunity occurred to measure the angle. December 14, 1873.

No. 187 = L 9397.

R.A. = $4^h\ 54^m\ 9^s$ Decl. = $+\ 14°\ 20'$

P = 270° ± D = 1″ Mags. 8 = 8.

A close double star on the borders of *Orion* and *Taurus*. It is 2^m following, and 1′ 44″ south of the wide triple, H. V. 113. The latter appears to be identical with H. V. 57 (= Sh. 49 = Σ C.P. 148). The new pair is an interesting object. January 11, 1874.

No. 188 = τ *Orionis*.

R.A. = $5^h\ 11^m\ 47^s$ Decl. = $-\ 6°\ 58'$

(A and D = H.V. 25 = Σ C.P. 167 A and B = H 2259)

A and B, P = 250°·4	A and D, P = 63°·8	B and C (new), P = 55° ±
D = 18″ ±	D = 18″ ±	D = 2″
Mags. 4, 14	Mags. 4, 12	Mags. 14, 16.

The duplicity of the smaller companion to τ *Orionis* was discovered with the Chicago 18½-in. refractor, December 22, 1873. I was examining the trapezium of *Orion* for the new stars alleged to have been seen within it, and upon directing the instrument to this star saw at once that the companion discovered by Sir John Herschel was double. The second star of the close pair is much smaller, and might easily have escaped attention from an observer not specially interested in double stars. I am not at all certain that the distance is as great as 2″, and it may not exceed 1″·5, but, not being very familiar with the eyepieces, my estimate is entitled to less credit than in the other pairs in this list. The measures of B and D, given above, are from Sir John Herschel's Fifth Catalogue of Double Stars, where this companion, as a single star, is first noticed. It is singular that it should have been missed by Herschel I., as with my 6-in. refractor it is very obvious

under almost any circumstances. My instrument, of course, fails to show the close star. I do not know how large an aperture would be necessary, but probably at least 12 or 15 in. would deal with it under the best conditions. The only similar object of the kind in the heavens heretofore observed is α^2 *Capricorni*. The duplicity of Herschel's 16 mag. companion to that star was discovered by Mr. Alvan Clark, while the 11½-in. refractor was in his possession (and independently, but with the same instrument, by myself at Chicago). Prof. C. A. Young was able to see the companion double, with the 9·4 in. Dartmouth College refractor, on the Rocky Mountain Expedition, at an elevation of about 9,000 feet above the sea. He estimated the distance of the components as 1″·5. This agrees nearly with my estimate at the time of finding it. I am not aware of its ever having been seen with any other instruments. Both of these minute double companions should be observed and measured, for there is every *appearance* of physical connexion, and they may prove as interesting as they are (with ordinary instruments) difficult. I may add here that I could not see the slightest trace of any stars within the trapezium. This is the second time I have examined it with the large telescope under favourable circumstances, when a star of one-tenth the light of the 6th star could not have been overlooked. The failure of this instrument, and the new 26-in. refractor, at the U. S. Naval Observatory, as well as other larger apertures, to show any of the stars claimed to have been seen with much smaller instruments, renders it almost certain that they do not really exist. The theory of variability cannot account for such failure where the object has been so carefully and frequently observed.

No. 189 = *Orionis* 81.

R.A. = $5^h\ 14^m\ 32^s$ Decl. = $-5°\ 29'$

P = 285°·0 D = 5″ Mags. 7, 15.

This is the *s f* of two bright stars about 1½° *n* of τ *Orionis*, The companion is excessively minute, and generally it is a very difficult object. When measured, however, it was very well seen, and the measures fairly accordant. The companion seemed to have a reddish tint. February 9, 1874.

No. 190 = *Orionis* 82.

(A and C = Σ 692 = H. IV. 87 = S 475)

R.A = $5^h\ 14^m\ 38^s$ Decl. = $-8°\ 9'$

P = 360° ± D = 0″·5 Mags. 8, 8.

The principal star of this wide pair is a very close and exceedingly difficult double. The elongation appeared to be very nearly in the direction of the distant star. The difficulty of the object fully accounts for it being overlooked by Struve, Herschel, South, and others, who have observed and measured A and C.

No change has taken place in that pair. Struve (1831·5) gave P = 4°·2; D = 34″·86; mags. 8·8. Engelmann (1863·1), P = 3°·9; D = 35°·58. February 9, 1874.

No. 191 = *Arg.* (+ 34°) 1033.

R.A. $5^h\ 17^m\ 19^s$ Decl. = + 34° 27′

P = 204° D = 2″ Mags. 9, 9.

A not very interesting pair of small stars $2^m\ 52^s$ *p* and 7′·8 *n* of Σ 707. The latter has a second minute companion, not mentioned by Struve. April 12, 1874.

No. 192 = τ *Aurigæ.*

(A and C = H.V. 21 = Σ C.P. 205)

R.A. = $5^h\ 40^m\ 52^s$ Decl. = + 39° 8′

A and B P = 360°	A and C P = 32°·5
D = 25″	D = 60″
Mags. 5, 14	Mags. 5, 12.

I am not acquainted with any measure of Herschel's companion made heretofore. The new companion is exceedingly faint, and rather less than half the distance of the other. It is too minute for reliable measures. January 4, 1874.

No. 193 = Weisse VI. 208.

R.A. = $6^h\ 9^m\ 10^s$ Decl. = + 4° 0′

P = 90° ± D = 15″ Mags. 8, 15.

Another very faint companion, and only seen by careful attention. There is a second brighter companion, 62″ distant, in the direction of 225°. January 11, 1874.

No. 194 = *Arg.* (+38°) 1537.

R.A. = $6^h\ 28^m\ 4^s$ Decl. = + 38° 6′

P = 283°·5 D = 1″ Mags. 8, 8½.

A very pretty pair, at first taken to be OΣ 147. That was entered in Otto Struve's Catalogue of 1850 with a distance of 0″·6, with two bright distant companions. Dembowski, in 1865, found the close pair single, as it is now. The star is perfectly round, with a power of 400. The new pair is $1^m\ 58^s$ *p* and 4′ *s*. The three stars in OΣ 147 form an equilateral triangle; the side being about 45″, and the stars being nearly the same brightness, it is a striking object. Possibly the pair I have noted was the one observed by Otto Struve, and in some way confounded with one of the stars of this wide triple. April 2, 1874.

No. 195.

R.A. = $6^h\ 37^m\ 7^s$ Decl. = $-23°\ 8'$

P = 214° D = 5″ Mags. 7, 12.

A very unequal pair is *Canis Major*. Measures not very good. March 4, 1874.

No. 196 = Weisse VII. 142.

R.A. = $7^h\ 6^m\ 27^s$ Decl. = $-5°\ 14'$

P = 200° ± D = 14″ Mags. 9, 10.

The last of a line of three stars in the same field; a rather faint and insignificant object. February 16, 1874.

No. 197 = L 14026.

R.A. = $7^h\ 7^m\ 0^s$ Decl. = $-6°\ 57'$

P = 150°·6 D = 2″ Mags. 8, 10.

A pretty pair in *Monoceros*, and readily seen. February 17, 1874.

No. 198 = L 14503.

R.A. = $7^h\ 20^m\ 36^s$ Decl. = $-20°\ 43'$

P = 212° D = 3″·5 Mags. 8, 11.

A very fine pair in *Canis Major*. The primary is decidedly red. The pair of 10^m stars 40^s *p* and 4′ *s* is H 3964. March 4, 1874.

No. 199 = L 14480.

R.A. = $7^h\ 19^m\ 57^s$ Decl. $-20°\ 56'$

P = 19° D = 1″·2 Mags. 7, 9.

An elegant double star just south of H 3964, mentioned in the note to the last. There is a distant 9 mag. companion in the direction of 301°. March 4, 1874.

No. 200 = 70 *Geminorum* = H. VI. 70.

R.A. = $7^h\ 30^m\ 41^s$ Decl. = $+35°\ 19'$

A and B,	A and C,	C and D (new),
P = 188°·3	P = 98°	P 249°
D = 98″	D = 160″	D = 1″·5
Mags. 6, 11	Mags. 6, 9½	Mags. 10, 12.

Sir William Herschel entered 70 *Geminorum* in his Class VI. of double stars without measures, with estimated distances of each 60″±. I do not know that these distant companions have ever been measured. The results given above are from my own observations. The most distant of the two companions is a close double of more than ordinary difficulty, and requires very

careful attention, even in good weather, to be well seen. The measures, though made with as much care as possible, may not be very close. February 17, 1874.

No. 201 = L 14945.

R.A. = $7^h 33^m 41^s$ Decl. = $-20° 0'$

P = 330° D = 2″–4″ Mags. 8, 8.

A very easy pair in *Argo*. The distance was not carefully estimated. April 10, 1874.

No. 202.

R.A. = $7^h 56^m 59^s$ Decl. = $-26° 53'$

A and B, P = 162° A and C, P = 248°

D = 6″ D = 15″

Mags. 7, 14.

A very unequal double, with a more distant companion, C, preceding. March 21, 1874.

No. 203.

R.A. = $7^h 57^m 40^s$ Decl. = $-27° 14'$

P = 246°·5 D = 5″ Mags. 7, 10.

A fine pair in *Argo*. There is a 9 mag. companion 64″ distant in the direction of 73°·7 (+180°?). Herschel has a double, H 4037 (*Cape Obs.*) not far from this place, which I could not find. The angle and distance are both larger than in this pair, so they can hardly be identical. March 22, 1874.

No. 204 = L 16074.

R.A. = $8^h 7^m 2^s$ Decl. = $+10° 45'$

P = 304°·8 D = 1″ Mags. 7½, 11.

An excessively difficult pair, 31′ 48″ *s* of the binary, Σ 1202. The closeness and great inequality of the components render this a very troublesome object to observe, except under favourable conditions. March 27, 1874.

No. 205 = Pyxis 9 (Bode).

R.A. = $8^h 27^m 54^s$ Decl. = $-24° 7'$

P = 130° ± D = 0″·5 Mags. 7, 7.

Very close and difficult, but finally pretty well seen. An attempt was made to measure it, but the definition was too poor at the time to make the results much better than an estimate. March 12, 1874.

No. 206.

R.A. = $8^h\ 30^m\ 17^s$ Decl. = $-24°\ 42'$

P = $278°·6$ D = $1''·5$ Mags. 8, 9.

Another fine pair in the same vicinity as the last. This is the following of two stars nearly in the same parallel, $1^m\ 22^s$ apart. March 12, 1874.

No. 207 = L 17091.

R.A. = $8^h\ 33^m\ 44^s$ Decl. = $-19°\ 19'$

P = $99°$ D = $5''$ Mags. $6\frac{1}{2}$, 11.

A splendid pair; the primary red, and companion apparently blue. February 16, 1874.

No. 208 = L 17103 = Pyxis 17 (Bode).

R.A. = $8^h\ 33^m\ 54^s$ Decl. = $-22°\ 16'$

P = $30°·4$ D = $1''·4$ Mags. 6, 9.

This exquisite pair was found in focussing the glass for No. 205. It is a naked-eye star, and rated 6 mag. in Argelander and Heis. There is a distant companion in the direction of $215°$; D=$72''$. Nos. 205, 206 and this pair are in the same neighbourhood, and were found within a few minutes of each other. March 12, 1874.

No. 209 = Weisse VIII. 849.

R.A. = $8^h\ 35^m\ 24^s$ Decl. = $+39°\ 14'$

P = $355°·5$ D = $1''·6$ Mags. 8, $8\frac{1}{2}$.

A pretty but very easy pair found in determining the correct place of Σ1259, the decl. of which in *Positiones Mediæ* is 10′ in error.

No. 210 = L 17696.

R.A. = $8^h\ 51^m\ 18^s$ Decl. = $-16°\ 58'$

P = $182°·5$ D = $2''·3$ Mags. $7\frac{1}{2}$, $7\frac{1}{2}$.

A pretty pair of nearly equal stars. February 16, 1874.

No. 211 = *Hydræ* 68.

R.A. = $8^h\ 55^m\ 42^s$ Decl. = $+3°\ 9'$

P = $255°·6$ D = $1''$ Mags. 7, 10.

A splendid, but exceedingly difficult pair, in the same field with Σ 1302. The components are close and very unequal, and the best circumstances are requisite for a separation of the discs. The measures were apparently good, and accordant. Σ 1302, an easy pair of small stars, is 43^s *p* and $4'\ 16''$ *n*. The new pair is L 17831. April 9, 1874.

No. 212 = *Hydræ* 95.

R.A. = $9^h\ 10^m\ 12^s$ Decl. = $-7°\ 51'$

P = $218°\cdot5$ D = $1''\cdot5$ Mags. 7, 9.

Another very elegant pair 23′ 32″ *n.* of 24 *Hydræ*, and 37^s *p.* The distance not very carefully noted, but measures good. The star is L 18296-7. February 4, 1874.

No. 213 = L 18648.

R.A. = $9^h\ 22^m\ 25^s$ Decl. = $-7°\ 34'$

P = $185°\cdot5$ D = $1''\cdot5$ Mags. 8½, 10.

This difficult pair is 34′ 31″ north of *α Hydræ*, and is a more difficult object than the last, as both components are much smaller. February 13, 1874.

No. 214 = L 19064 = *Felis* 15 (Bode).

R.A. = $9^h\ 35^m\ 52^s$ Decl. = $-17°\ 56'$

P = $263°\cdot9$ D = $2''\cdot5$ Mags. 7½, 11.

A very pretty pair, and not difficult. March 12, 1874.

No. 215 = *Lacaille* 4058.

R.A. = $9^h\ 48^m\ 41^s$ Decl. = $-27°\ 26'$

P = $340° \pm$ D = $1''\cdot5$ Mags. 7½, 11.

An elegant, but excessively unequal and difficult double star. Occasions when so severe a test can be well seen so far south are comparatively rare, and no opportunity occurred after its discovery to obtain a satisfactory measure of the angle. March 26, 1874.

No. 216 = *Lacaille* 4074.

R.A. = $9^h\ 51^m\ 20^s$ Decl. = $-25°\ 59''$

P = $177°$ D = $2''\cdot5$ Mags. 6½, 13.

This is one of the most difficult stars of its class in this list. The companion is so minute a point as to require the most careful attention. It is more difficult than the last, which was found a few minutes before on the same evening; but measures were taken at once of this pair, when it was seen much better than at any subsequent time. The places of both are taken from the Washington *Catalogue of Stars.* March 26, 1874.

No. 217.

R.A. = $10^h\ 1^m\ 16^s$ Decl. = $-24°\ 8'$

P = $273°\cdot5$ D = $1''\cdot5$ Mags. 7½, 7½.

Another very fine pair, found the same evening as the two

preceding. The stars are nearly equal, and being only moderately close, it is much less difficult than the others. Place from the Washington *Mural Circle Zones.* March 26, 1874.

No. 218 = L 19765.

R.A. = $10^h\ 1^m\ 40^s$ Decl. = $-19°\ 7'$

P = $109°·5$ D = $1''$ Mags. 8, 8.

The preceding star of a small equilateral triangle. A beautiful, but difficult pair. March 10, 1874.

No. 219 = *Felis* 54.

R.A. = $10^h\ 15^m\ 57^s$ Decl. = $-21°\ 56'$

P = $193°·6$ D = $2''$ Mags. 7, 9.

Another elegant double in the same neighbourhood. The $10''$ pair of $8\frac{1}{2}$-in. stars about $4'$ south is H 4303 (*Cape Obs.*) This star in Bode 6 mag. March 10, 1874.

No. 220 = *Crateris* 22.

R.A. = $11^h\ 6^m\ 34^s$ Decl. = $-17°\ 51'$

P = $147°·7$ D = $0''·5$ Mags. 6, 6.

A most elegant double star is *Crater*. It is given as 5 mag. in Bode and Lalande, but 6 mag. in Argelander and Heis. The angle given is a mean of two sets of measures, fairly accordant, considering the closeness of the pair. In Bode this star is designated ψ *Crateris.*

No. 221 = L 24532.

R.A. = $13^h\ 6^m\ 54^s$ Decl. = $-14°\ 49'$

P = $49°·4$ D = $1''·5$ Mags. 8, 10.

This unequal pair is $43'$ north of 53 *Virginis*, and is moderately difficult. March 26, 1874.

No. 222 = L 24636.

R.A. = $13^h\ 10^m\ 55^s$ Decl. = $-20°\ 54'$

P = $10°$ D = $1''·5$ Mags. 8, 10.

Another pair in *Hydra*, similar in distance and magnitude to the last. March 26, 1874.

No. 223 = L 25350.

R.A. = $13^h\ 38^m\ 58^s$ Decl. = $-2°\ 43'$

P = $342°·8$ D = $18''$ Mags. 8, 11.

A wide pair, found in looking for a double near the same place, discovered by Winnecke (*Astron. Nach.* 1738). The latter

is a pair of $9\frac{1}{2}$ mag. stars, $4''\cdot68$ apart. The distance seems to be rather less now, and if a rough measure of the angle is not considerably in error, it has diminished since Winnecke's measures in 1855. May 12, 1874.

No. 224 = Weisse XIV. 95.

R.A. = $14^h\ 7^m\ 37^s$ Decl. = $+13°\ 8'$

P = 73° D = $0''\cdot7$ Mags. $8\frac{1}{2}$, $8\frac{1}{2}$.

An excessively difficult pair in *Boötes* 35′ north of O Σ 269. Measured, but results disagree much. May 6, 1874.

No. 225 = χ Turdi Sol.

(A and B = H N. 80 = Sh. 179 = Σ C.P. 459)

R.A. = $14^h\ 7^m\ 47^s$ Decl. = $-19°\ 26'$

A and B (South),	B and C (new)
P = $295°\cdot8$	P = $103°\cdot4$
D = $35''\cdot12$	D = $1''\cdot5$
Mags. 7, $7\frac{1}{2}$	Mags. $7\frac{1}{2}$, 9.

It is remarkable that the duplicity of B should have escaped the Herschels, South, Jacob, and others, who have observed the wide pair, if it was then as easy as it is now. It could hardly be missed now if examined with any instrument of not less than 4 or 5-inch aperture, under favourable conditions. The wide pair is evidently fixed. Jacob (1847·7) found P = $295°\cdot7$; D = $35''\cdot23$. For the same pair I obtained, P = $295°\cdot8$. The measures of the close pair are very satisfactory, and the result of two nights' observation, the first giving $103°\cdot3$, and the second $103°\cdot5$. This star is L 26319-0. May 12, 1874.

No. 226 = L 26665.

R.A. = $14^h\ 32^m\ 4^s$ Decl. = $-21°\ 49'$

P = 70° D = 1″ Mags. $7\frac{1}{2}$, $7\frac{1}{2}$.

A fine but close and difficult pair in *Scorpio*. May 13, 1874.

No. 227 = B.A.C. 5039.

R.A. = $15^h\ 12^m\ 7^s$ Decl. = $-23°\ 50'$

P = $184°\cdot1$ D = $1''\cdot7$ Mags. 7, $10\frac{1}{2}$.

This fine pair is the middle one of three stars in the same field, and nearly following each other. This was found in looking for H 4759, a suspected close pair. (*See note to next No.*) May 6, 1874.

No. 228 = B.A.C. 5041 = H 4756.

R.A. = $15^h\ 12^m\ 38^s$ Decl. = $-23° 50'$

P = 329° D = 0″·8 Mags. $7\frac{1}{2}$, $7\frac{3}{4}$.

Herschel has entered this pair in *Cape Observations* as follows: P = 180° ±; D = $\frac{3}{4}$″; Mags. 9·$9\frac{1}{2}$ with the note "Requires verification." On the first examination I took No. 227 to be this star, with a considerable error in Herschel's distance, but upon observing the following star again, I saw at once that it was a close pair, and undoubtedly the one suspected by Herschel, although the magnitudes and position-angle do not correspond very closely, and his decl. is 1′ too small by the place in B.A.C. and Washington *Catalogue*. It is singular that he should have missed the other pair which is in the same field, 31^s *p*. May 12, 1874.

No. 229 = L 45726.

R.A. = $23^h\ 14^m\ 26^s$ Decl. = $+56° 35'$

P = 37° D = 12″ Mags. 8, 11.

A wide and unimportant pair in *Cassiopeiæ*. January 15, 1874.

Chicago, 1874, *May* 12.

Note on the Double Star, Σ 707. By S. W. Burnham, Esq.

Dembowski, in 1864, in his measures of Struve's double stars, observed this pair, as he supposed, but found two pairs near each other, of which his measures are as follow:—

	°	″		
Preceding in R.A.	P = 51·6	D = 28·62	Mags. 9·5	10·5
Following in R.A.	P = 328·9	D = 20·88	Mags. 9·3	10·0

The measures of Σ 707 in the *Mensuræ Micrometricæ* are P = 131°·6; D = 18″·29; Mags. 8, 10·2. Dembowski has this note:—"On voit deux doubles dans le champ de la lunette, à peu de distance l'une de l'autre. Il y a certainement un fort changement, mais laquelle des deux doubles est la Σ 707 mesurée par Struve?" (*Astron. Nachr.* 1572). Obviously Struve's pair could not be either of the stars observed; and upon a recent examination I found that Dembowski's pairs are in a low power field with Σ 707, the following one of the two being about 38^s preceding. These stars are all too faint and wide to have any special interest. I found a second companion to Σ 707, not noticed by Struve. It is

excessively minute, and would be at least 11·5 mag. of Struve's scale. I estimated the angle at about 60° more than the other companion, and the distance a little more than 5″ greater, giving approximately, P = 190°; D = 25″. A single measure of Struve's companion, and Dembowski's following pair, for the purpose of positive identification gave 133° and 328° respectively. The new companion is a difficult object for my 6-inch aperture, but two measures were obtained giving P = 196°. This star is not in the *Positiones Mediæ*. I found that it was identical with Weisse V. 518 (9 mag.)

In examining the neighbouring stars on this occasion I found a much closer pair than either of those mentioned, 2^m 52^s preceding, and 7′·8 north of Σ 707. This is *Arg.* (+ 34°) 1033, and given in that catalogue as 8·9 mag., while Σ 707 is rated 9·1 mag. To me it appeared brighter than either of the stars measured by Dembowski, but a little fainter than Struve's. The distance I estimated as 2″, and a mean of five measures of the angle gave 202°. In the field a short distance following is a 10″ pair of very minute stars.

Chicago, 1874, April 24.

Note on the Double Stars, Σ 1801 *and* O Σ 265. By S. W. Burnham, Esq.

Dembowski, in his measures of Struve's double stars (*Astron. Nachr.* 1736) has this note to Σ 1801: "Aucun satellite—et pour la deuxième fois" (1865·4). A few evenings since I looked this up, and found it without difficulty. It is a faint and insignificant pair, but now a very easy object with my 6-inch refractor. Struve gave P = 64°·5 : D = 18″·44 : Mags. 9, 10·5. A rough measure of the angle for the purpose of certain identification made it 66°. The magnitudes of the components appeared to be about as estimated by Struve. It would seem that the companion is variable, unless by some accident Dembowski looked in the wrong place. About 2^m 11^s following this pair and 4′ south is a similar but rather brighter pair, which at first I took to be the one in question, with a considerable change in position-angle, but found to be identical with H 2701. Herschel gives no angle, but distance as 15″. A mean of 3 measures resulted in finding P = 36°. The only other observations or mention of Σ 1801 with which I am acquainted are by Sir John Herschel, who examined it twice, and noted it in his Fourth catalogue, and once in the Fifth catalogue. In the former his magnitudes are 9, 10; in the latter, 9·10, 11.

The double star O Σ 265 is also noted by Dembowski, "point de

satellite" (1866·3). This is one of the pairs excluded by Otto Struve in the Catalogue of 1850. I see the companion now, although not without some difficulty. In the Catalogue of 1843 the magnitudes are 7, 10, and distance 12″. I do not know of any measures of the position-angle. It is very nearly preceding, or about 270°, and is certainly much smaller than 10 of Struve's scale.

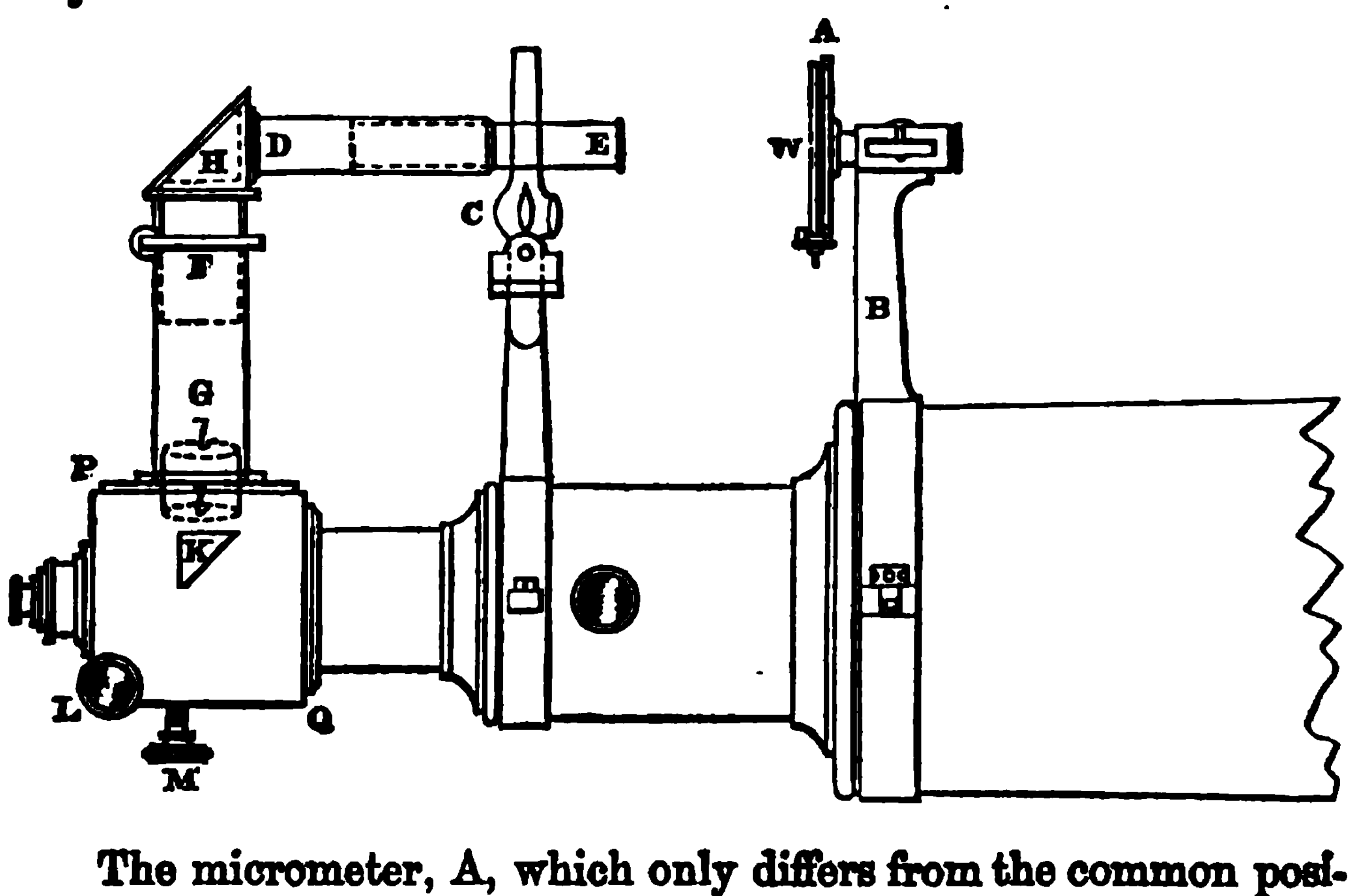
Chicago, 1874, *May* 17.

On a New Form of Position Micrometer.

By G. P. Bidder, Esq., Q.C.

The accompanying diagram represents a form of position micrometer, designed more particularly for the measurement of faint objects.

The principle of its construction is to throw into the field of view, by means of reflectors, the image of illuminated wires; so producing the appearance of bright wires on a dark field. For purposes of differential measurement, this arrangement affords many advantages over the ordinary dark field micrometer (in which the wires placed in the field are illuminated by side-light), and is, I believe, greatly superior in the measurement of faint objects.



The micrometer, A, which only differs from the common position micrometer in having no eyepiece before the wires, is placed above the telescope, and attached to it by a firm support, B. The wires, W, are illuminated by a lamp, C, in front of them, which is carried on an upright rod attached to the telescope, and is

made capable of being shifted in height and inclination as may be required in order to throw the light on to the wires.

Opposite the wires is a tube, D E, parallel to the telescope, which excludes stray light. A diaphragm at the end nearest the wires reduces the aperture, and assists in effecting the same object.

This tube is attached to a short tube at right angles to it, which fits into the tube F G. This last tube is perpendicular to the axis of the telescope, and is fixed to the square box, P Q, forming a prolongation of the telescope draw-tube. At the intersection of the axis of the two tubes, D E and F G, is placed a rectangular prism, H, and within the tube F G a pair of convex achromatic lenses, *l l*. Below these is a second rectangular prism, K, so placed as to be above the cone of rays passing from the object-glass to any point in the field of view. The light from the wires being thrown by reflexion at the first prism on to the lenses, these form an image of the wires, which, by adjustment of the position of the lenses, is made after reflexion at the lower prism to coincide with the principal focus of the object-glass.

The distance from the wires to the lenses being greater than from the lenses to the focus, the image is reduced in size (in my micrometer the linear dimensions are $\frac{2}{9}$ of the dimensions of the wires themselves). By this means great delicacy of measurement is obtained.

The wires are seen in the field side by side with the stars to be measured, and may be superimposed upon them, but, being mere images, cannot hide them. The light can of course be reduced until the wires are scarcely visible. In the case of excessively faint stars an additional contrivance may be employed, by the help of which the faintest visible objects can be measured. It consists of an opaque bar fixed across a short tube, and inserted at the end E of the tube D E, and as near as conveniently may be to the wires. The bar is placed in such a position as to be transverse to the measuring wires. The effect is to intercept the light from a portion of each wire, and so produce a dark gap in the image of each. The image of the wires being moved in the field so that the stars to be measured appear in these gaps, they can then be readily measured. The lower prism is so mounted that by turning the screw-heads, L M, it can be tilted or turned in any desired direction, by which means the image of the wires may be moved to any part of the field. The whole instrument is so arranged that the screwheads of the micrometer are within reach of the observer while the eye is at the eyepiece.

By the interposition of coloured glass between the lamp and the wires they may be coloured with any desired tint.

It is essential to the accuracy of the measurements that the position of the micrometer and of the lenses, and the distance between them, should not be varied when once adjusted.

I am indebted to Mr. John Browning, who has made my instrument, for the admirable manner in which he has carried out

my design. The instrument, so far as I have hitherto tried it, fully answers my expectations.

A plan somewhat similar to Mr. Bidder's for using a "ghost" of the wires instead of the wires themselves, but applied to a meridional instrument, is described by M. Karl v. Littrow, in the *Proceedings of the Academy of Sciences*, Vienna, Vol. XX., for 1856. M. Littrow uses intercepted wires in the same way as in Mr. Bidder's contrivance.—ED.

On the Fixing of Spider-lines in Collimators and Transit Telescopes. By Capt. John Herschel, R.E.

Some time ago I had occasion to replace some broken wires in the diaphragm of a transit telescope. As it is an operation which every practical astronomer ought to be able to perform for himself—more especially when on foreign duty—and as there *are* difficulties about it which may be lessened by reading an account of a similar operation, of a rather unusual kind, as actually performed, I will endeavour to recall what I can recollect of it.

I may preface this particular account with one or two general remarks upon the subject.

The occasions upon which skill in wiring diaphragms may be brought into play with advantage are so numerous, that it is well to take advantage of every opportunity to put whatever may exist into practice, with a view to prepare for cases of emergency. It does not always happen that the wires of a collimator, for instance, are adapted, in thickness and angle, to the use to which they are to be put—in which case it is very convenient to be able to replace them readily. On foreign service, too, the liability to disfigurement or fracture is much increased; and if confidence has been acquired, a few hours' work at the right time will save much annoyance later. The case I am about to describe was so prefaced, fortunately, by many lesser experiences.

My next remark has reference to ways and means. There are often many ways of doing the same thing, and these often depend on the means at hand. It will be seen that mine were not of an out-of-the-way kind. Such as they were, they sufficed, but possibly others would have served equally well. Some implements, however, are almost essential; for instance, a camel's hair or sable brush—better still, two or three; a lump of beeswax; a few corks, and a few carpentering tools and materials are also useful. Some sort of varnish, too, is necessary—I always use shellac—and some spirits of wine.

The diaphragm upon which I operated was one cut for 25 wires in 5 tallies of 5—the whole occupying about one-third of an inch—on the longer sides of a rectangular plate or frame

about $1\frac{3}{4}$ inch $\times$ $3\frac{1}{2}$ inches. The occasion arose, let us say, after the visit of some obnoxious creature. Whatever it was, the set was damaged, and I concluded to try my hand at a new one.

Now arises the question whether it is better to replace individual wires or the whole set. Considering the relative difficulties —upon which I will not now waste space—I caution anyone against partial reconstruction—especially in delicate cases. At any rate, I decided the question by pushing a finger through the frame.

The next step was to clean the frame. This is very important. Unless all the old fibres and the varnish are removed from the grooves, there will be trouble afterwards. Here the brush and spirit, and a little patience and care, come into play. A cloth may also be applied without fear. It is not an easy thing to scratch brass so deeply with cotton fibre as to form false grooves. Of course the less sand there is in the cloth the better; but there may be a good deal of misplaced tenderness in handling a wire-frame after its wires are broken or condemned.

We proceed now to get the fibre. A good deal might be said about spiders with reference to the fitness of their silk. I do not know enough to speak positively. But I *believe* that very generally what is to all appearance a simple thread is really a bundle of parallel threads. Whether it is possible to get a simple fibre of any considerable length and of sufficient thickness, I doubt. As a fact I am pretty sure that my 25 wires, when finished, were a series of equal strands of fibres—although in appearance as perfect a set as I have ever seen—infinitely superior, I need hardly say, to those which were now beyond the reach of comparison.

The size, uniformity, and quantity of web required are all matters for consideration. The size must be found by trial. Different species of spiders give very different qualities, and in very different degrees. For the sake of uniformity it is very desirable to secure an ample supply from the same source. The quantity will depend on the method of using it.

Having selected my spider, I proceeded to spin from him thus. I had prepared a pane of window-glass with a rod at the back, attached by lumps of wax, to act as a spindle. Having coaxed the spider to attach his web to this, and having shaken him off, I allowed him *to alight*, and then reeled off just fast enough to prevent him from attaching again. I cannot enter into his feelings sufficiently to explain the why and the wherefore, but after many trials I found this the only safe way to get a *long uniform web*, and in this way I generally *could* get an unbroken thread of 30 or 40 feet without stopping. It will be found that very rapid reeling is necessary, and this the 8-inch window-pane spinning on its temporary axle sufficiently provides for. It is hardly necessary to say that, except in a peculiar light, the whole operation is effected without *seeing* the thread. The attitude of the spider alone betrays the connexion; and when, by watching the

creature, the rupture of this connexion becomes known, the direction of the axle must be immediately changed, so that the last two or three turns of the web may cross the glass at an angle—otherwise great difficulty will be experienced in finding the end.

It may seem absurd to wind off so much when only a few inches are wanted. I reply that I wanted 12 or 15 feet, and when my work was done actually had at least 10 feet of continuous thread employed upon the diaphragm.

The next operation introduces a device which is the principal feature in my plan. As yet the thread is wound anyhow. I want to wind it afresh in an orderly manner. For this purpose I make a little hook of light wire, which is to hang on the web as a man hangs with two outstretched arms from a horizontal bar. It is intended to run freely along the web, or the web through it, without risk of the latter twisting upon itself. I will call this the *stirrup*.

The end of the web being found, and one turn unwound, the stirrup is hung on to the slack, and the end attached by a piece of wax to the glass. By this time the need of two Y supports for the spindle will be found, if not already provided. When placed upon them the web will form a swing as it were for the stirrup, and as the spindle is turned the fibre will be wound up on one side as fast as it is unwound at the other, and this winding can be directed by tilting, to any required extent. By this means the whole can be passed under review and re-wound, backwards and forwards, with composure and certainty.

The next step was to transfer the web to the diaphragm. For this purpose I attached a spindle lengthwise at the back of that too, and supported it alongside the other. Then, having transferred one end of the web to the brass frame, the stirrup swinging between, I was able to wind off upon it as much as was wanted, viz. from 30 to 40 turns of the frame, and then, having broken it off from the glass, attached the other end also to the diaphragm at the other end of the rectangle, the stirrup still swinging in the light below. The arranging process was now repeated preliminary to a final re-winding upon the grooves.

This winding and re-winding has the effect of insuring the detection of flaws and of equalising the tension.

The re-winding upon the grooves requires steady supports and a slight tilting arrangement; and the placing of every successive convolution in its place, when the face of the diaphragm is vertical, must be done with the help of a lens held in an appropriate support in front of it.

Ultimately the whole of the 25 convolutions having been thus arranged as nearly as practicable in or close to their respective grooves, the outer edges of the diaphragm were streaked with varnish; and when dry the superfluous fibres at the back cut off, by running a knife-edge along the angles of the frame.

At this stage a final examination of the wires might have

led, but did not, to the substitution of one or more from the unused stock. The perfection of the method is such that it can hardly happen that any such substitution would be necessary.

The last step is the most troublesome. Every fibre has now to be lodged in its groove. This may be done approximately first—always with a brush—and a very weak varnish applied. The obstinate ones can then be manipulated and coaxed with greater freedom. And, lastly, a stronger varnish fixes them all.

I do not know how these things are done by the professional opticians. But I do know that, both in this case and in a second, the intervals between the wires were less unequal after the operation than before. The improvement in the uniformity of the wires as to thickness might be a matter of opinion, but the greater regularity is on record, numerically, as a fact. I infer from this that the method is surer, if not easier.

Observations of the Total Solar Eclipse of April 16, 1874, at Klipfontein, Namaqualand, South Africa.

By E. J. Stone, Esq.

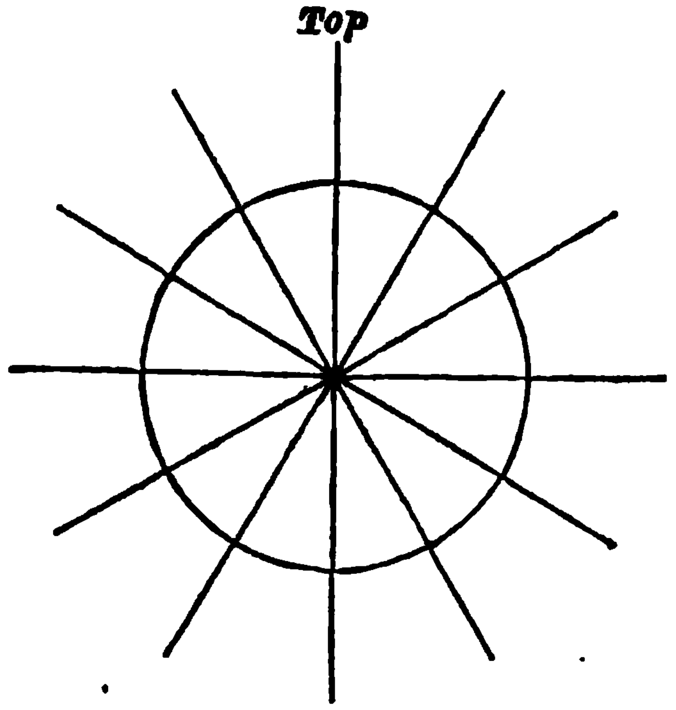
(*Extract from a Letter to the Astronomer Royal.*)

I observed the eclipse from Klipfontein, a station about 3,000 feet above the sea-level. The sky was perfectly clear, and no finer day could have been wished for. I had borrowed a four-inch telescope, mounted as an altazimuth, from Mr. H. Solomon. My spectroscope was one with two dense flint prisms of 60°; a fair amount of dispersion, therefore, being thus obtained. My great difficulty was to attach the spectroscope firmly to the telescope. Ultimately I was obliged to give up all idea of using the prism of comparison, and to fix the tubes together by wrappers of wash-leather. In this way I secured a very firm connexion between the spectroscope and telescope. I placed two wires in the focus of the telescope of the spectroscope for estimations, and determined to measure only the position of one line in the Corona, the micrometer wire being left untouched until the reappearance of the Fraünhofer lines, when the differences between the line measured and these lines could easily be fixed.

The slit was set as wide as would allow of a clear and distinct view of the Fraünhofer lines. This I did because I expected to find the spectrum of the Corona faint, but was anxious to see whether the Fraünhofer lines were present or not in the spectrum of the Corona. I could not change the width of the slit without taking the spectroscope off the telescope, on account of the way in which I had been compelled to join them together.

During the partial eclipse I most carefully examined the speculum near the Moon's edge, by comparing it with the spectrum away from the Moon's edge, to see whether any fresh

lines were produced by any absorbing medium around the Moon, but I could not find a trace of any difference in the spectra near the Moon's edge and away from it. My slit was so placed that it was parallel to a tangent at the point of last appearance of the sun-light; and as the totality approached, my wife, by the aid of the finder, kept the Sun's limb about half-way across the slit. At the instant of totality the whole field appeared full of bright lines; but I had scarcely time to begin counting these lines before the greater number of them vanished, and the spectrum resolved itself into little more than that of hydrogen gas. Not wishing to spend the few precious moments available upon the spectrum of the prominences, I determined to see what the brightness of the Corona really was before turning the telescope upon it. This was the only view of the eclipse I allowed myself apart from the spectroscope, and I probably spent half a minute lost in admiration of the scene presented to me. The rose-coloured prominences reached very nearly all around the Moon's limb. My wife says there was one, and only one complete apparent break in the continuity. The height varied, of course, very considerably at different parts of the Sun's limb. The Corona was much brighter than I expected to have found it; but its constitution appeared to me uniform throughout, except that its brightness was less as you proceeded further from the Sun. I have a drawing of the Corona by Miss Alice Hall, which in my opinion, and in that of my wife, who observed the eclipse through a pretty good finder, very correctly represents the Corona as seen from our station. I have also a drawing made about 500 miles away, which agrees in all the principal points with that by Miss Hall. I believe that there cannot be a doubt, from these drawings, that the great features of the Corona were identical as seen from these two distant stations. One or two smaller drawings also agree in the principal features, and these drawings are, I think, more to be trusted than usual, from my instructions to refer the prominent points to sectors of 30° having been followed. Diagrams like the rough figure were prepared, and the line top and bottom adjusted so that it was the section of the paper by a vertical plane as defined by a string with a weight. I believe from the comparisons made, and what I myself saw, that the whole Corona, as seen in South Africa, was a solar appendage. Returning now to my own work —On first moving the telescope away from the prominences, I certainly saw two faint lines less refrangible than E, and one very bright line not far from E; but on moving the telescope more



away from the limb of the Sun I appeared to have lost those fainter lines, and I did not subsequently see them. The spectrum of the Corona appeared to consist of one very bright line and an ordinary sun-light spectrum. I feel perfectly convinced that, although from the faintness of this sun-light spectrum the lines could only be seen with some difficulty, that the Fraünhofer lines were present in the spectrum of the Corona. I examined this point again and again, and I feel more than ever certain upon the point, from my wife's recollection of my strong expression of opinion upon it at the time I was making the examination.

Having carefully looked to this point, I unclamped the spectroscope, and swept over the whole spectrum from the extreme red to the extreme violet; but, independently of the bright line referred to before, I could not at this time see any traces whatever of bright lines. My instrument was then again clamped, and the bright line most carefully bisected by the micrometer wire. I then re-examined the spectrum for Fraünhofer's lines, but without touching the micrometer or moving the telescope even, until the total eclipse was over and the Fraünhofer lines appeared again in all their distinctness. The micrometer was then read, and the coronal line referred to two known lines near it by the micrometer. The wave-length of this bright line agrees so closely with that given by Young, that I could not with my dispersion answer for so small a difference. I am perfectly certain about the numerous lines seen in the spectrum close to the Sun's edge at the commencement of the totality, but the strata giving this spectrum must lie very close to the photosphere, for they were almost immediately covered by the advancing limb of the Moon. I am not prepared to say that the line spectrum of the Corona did consist entirely of one bright line, for, as I have said, I did see three lines near the Sun's limb in the brighter part of the Corona; but I am prepared to say that in the spectrum of the Corona, at some distance from the Sun, and away completely from the red prominences, there was no line in the spectrum of any degree of brightness except the one measured, and that there certainly was in addition to this an ordinary sun-light spectrum with Fraünhofer lines. I presume this spectrum must arise from the reflexion of the sun-light from the gas giving the line spectrum, and that we thus account for the polarisation of the light of the Corona in the plane through the Sun's centre. The natives were much afraid, and went to their huts. They got up a tale that I had brought the eclipse with me, and was looking for a missing star. Independently of the eclipse, I have made magnetical observations at four stations in Namaqualand, one at the Orange River.

Royal Observatory, Cape of Good Hope.
1874, *May* 11.

Note on Drawings of Jupiter made at Parsonstown, and by M. Terby, at Louvain, in 1873. By the Earl of Rosse.

In the Notes which accompany the chromolithographs of drawings of the planet *Jupiter* made at Parsonstown, which appeared in the *Monthly Notices* for last March, it is suggested that a disagreement as regards position between a marking shown on that of March 7, and a similar one very prominent on M. Terby's of March 8, $6^h\ 35^m$ to $6^h\ 50^m$, which we took to be identical with it, might possibly be traced to an error of 1^h in M. Terby's recorded time.

M. Terby having since written to me to say that he feels certain that no such error was made, it seems only due to him that the substance of his remarks should appear in the same publication.

We must now conclude that the feature drawn by M. Terby was exceedingly transient. It does not with certainty appear on a sketch by Mr. N. E. Green, dated March 7, 11^h, which he has been good enough to let me see; and if it can be identified with that which has been seen by us, it must be (as M. Terby suggests) with the marking towards the left of our No. 15, or near the centre of our No. 18.

The repetition of the same feature, with in some cases only exceedingly slight variation, in different parts of the same belt, together with the rapidity with which changes in their shape take place, often renders their certain identification impossible.

Extract from M. Terby's Letter, dated Louvain, May 1, 1874.

" Je puis vous assurer qu'il n'y a pas d'erreur dans le temps de mon observation du 8 Mars, de $6^h\ 35^m$ à $6^h\ 50^m$. J'en ai la certitude non seulement par l'inspection de mes notes, mais par le souvenir que, par une circonstance fortuite, j'ai gardé de cette observation. Je me rappelle parfaitement la bande recourbée qui est renfermée dans ce dessin. De plus, cette observation a été faite dans les mêmes conditions que la suivante de $8^h\ 10^m$ à $8^h\ 35^m$, que vous avez trouvée exacte, et représentant le crochet dont vous vous occupez. Je vois un autre moyen d'expliquer l'écart que vous signalez et je crois en trouver la confirmation dans vos intéressantes observations elles-mêmes.

"D'après vos calculs, la longitude jovicentrique de la tache en question doit être 260°. Or envisageant votre dessin 3, dans lequel la courbure dont il s'agit occupe la même position que celle de mon dessin 3, je vois que vous avez trouvé pour longitude de votre méridien central 256°·1, et pour le mien 223°·4. Il me semble impossible, d'après cela, d'admettre que les deux dessins représentent la même tache. La bande courbée de mon dessin 3 doit *précéder* celle que vous considerez spécialement, et est devenue invisible dans mon second dessin du 8 Mars, peut-être à cause de sa proximité du bord. Il est bien fâcheux que

jusqu'ici je ne connaisse pas d'observations coincident avec mon dessin de $6^h\ 35^m$; je suis persuadé que l'on y verrait la bande que j'ai représentée.

"A défaut de coincidence parfaite, je m'appuierai sur vos dessins 15 et 18, qui renferment des traces évidentes de bandes courbées, précédant celle dont vous vous occupez, car la longitude est ici 200°·1 et 220°·6.

"Ces prolongements établissent une communication entre le bord S de la bande équatoriale et des régions plus septentrionales, semblant avoir été très fréquentes en 1873; on en trouve un autre exemple dans les crochets de mes dessins 2, 9 et 11, que j'avais jugés visibles à peu près sur le même hémisphère, et qui, d'après vous, seraient complètement distincts."

June 8, 1874.

Observations of Jupiter, 1874. By E. B. Knobel, Esq.

Observations of *Jupiter* were commenced on February 8th, 1874, and up to the present time but few opportunities of fair observation have been lost. On the whole the opposition this year has been a very favourable one for study of the planet; a large number of fine nights, coupled with good definition, enables one, I think, to submit one's results with some amount of confidence.

The series of 35 drawings, which I beg to present to the Society, has been made with an 8½-inch silvered glass reflector by Browning, of excellent quality. The powers used were Browning-achromatic eye-pieces of 144, 208, and 250. Every care was exercised to represent what was actually seen, and to give the relative intensity of the various features; and with a view to obtain as impartial evidence as possible, no comparison of the drawings was made till they were all (with two exceptions) copied out in their present form.

The changes in the planet's aspect since the last opposition have been so numerous, that I have included in the series nearly all the drawings I was able to make. It might be thought that in such a series of drawings, many of which represent the same zenographical meridians, some would be merely replicas of others, but I have found that this is not the case. The state of the atmosphere has made definition variable, and, coupled with absolute changes in the features of the belts, renders it almost impossible for two drawings of *Jupiter* having the same longitude, but executed at different periods, to correspond exactly. Some of the principal features seem to have been permanent for the whole period of observation; others it is difficult to reconcile with what was observed on the same meridian at a subsequent date.

I have arranged the drawings in order of date, but to facilitate comparison, they are given in the following table in order of rotation, the longitude of the centre of the disk being calculated from the tables published in the *Astronomical Register.**

To eliminate as much as possible discrepancies which may be due only to imperfect definition, I append a column in which the number of crosses indicates *very roughly* the state of the atmosphere, in a scale of 4 degrees, as recommended by Dr. Lohse in his circular of last year:—

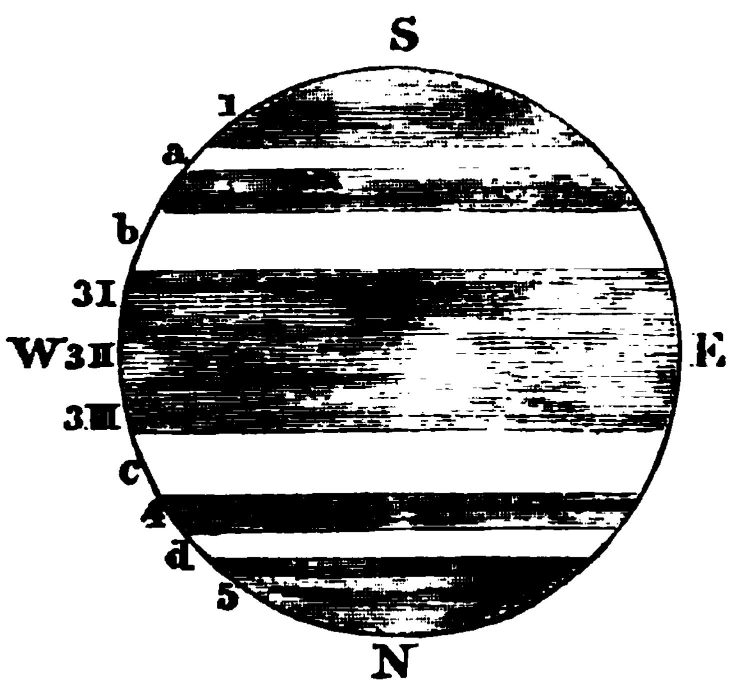
X	= Definition.	Very good, very sharp and steady.
XX	= ,,	Good and steady, but not continuously so.
XXX	= ,,	Tremulous; only momentary good views.
XXXX	= ,,	Bad, not sharp, though principal features discernible.

No. of Sketch	Date	h m		Longitude	Definition
20	April 5	8 20	G.M.T.	3	X
17	March 26	10 25	,,	12	XX
31	April 29	8 40	,,	31	X
6	March 7	10 45	,,	41	XX
18	March 31	10 40	,,	54	XX
13	March 24	10 15	,,	64	XXX
7	March 7	11 25	,,	65	XX
34	May 16	8 45	,,	74	X
24	April 12	11 0	,,	75	XXXX
21	April 5	10 40	,,	88	XX
14	March 24	11 0	,,	92	XX
25	April 20	8 13	,,	99	XX
22	April 8	8 40	,,	107	X
8	March 7	13 40	,,	147	X
15	March 25	8 30	,,	152	XX
23	April 8	10 10	,,	162	XX
32	May 2	10 33	,,	190	XX
16	March 25	10 0	,,	206	X
5	March 6	10 15	,,	232	XX
11	March 18	10 25	,,	246	X

* It would seem that there is either some error in the rotation period of *Jupiter*, assumed by Mr. Marth in these tables, or else considerable proper motion in the features of the planet. Sketches Nos. 20 and 35 correspond as far as the large white marking in the Equatoreal zone is concerned; but the longitude of the centre of No. 20 is 3°·0, and that of No. 35 is 330°·0, showing an apparent error of nearly 1 hour in time, whereas I have every reason to believe there is no serious error in the time of any of the drawings.

No. of Sketch.	Date.	h m	Longitude.	Definition.
4	March 1	11 45 G.M.T.	253	XX
26	April 21	8 30 „	260	XX
2	February 17	12 30 „	272	X
28	April 23	10 30 „	274	XX
12	March 23	10 30 „	283	XX
29	April 23	11 0 „	292	X
10	March 11	11 10 „	299	X
3	February 17	13 45 „	317	X
30	April 28	11 0 „	325	X
27	April 21	10 18 „	326	XX
33	May 8	9 17 „	329	X
35	June 1	8 57 „	330	XX
19	April 5	7 25 „	330	X
1	February 8	12 20 „	349	X
9	March 9	11 0 „	351	XXX

For the purpose of comparison with the series of chromo-lithographs of *Jupiter* given by Lord Rosse in the *Monthly Notices* for March, I beg to make use of his diagrams of the belts, in which the dark bands are designated by figures, and the light spaces by letters.



Zone 5. The North Polar zone has appeared about as dark as last year, and in 28 of the sketches it is shown separated from the North Temperate belt 4. In the others no division can be detected between 4 and 5. In seven of the sketches a narrow belt darker than 5 is shown lying between it and 4. This feature was not observed at all last year. On some nights this narrow belt was seen merely as a thin dark edge to the Polar zone, and it was along this that Satellite III. transitted on March 25.

Zone d has appeared much narrower than last year, and, as mentioned above, on many nights could not be perceived at all.

Zone 4. The North Temperate belt, which has been so conspicuous for the last few years, has appeared on some nights exceedingly faint, and on others singularly darker and more distinct. It has been said to have entirely disappeared this year, but on every occasion my drawings show its existence. Sometimes no separation could be detected between it and 5, but on

those nights it was clearly marked as a broad darker edge to that zone. In sketches 2, 3, 29, and 33 the north edge of this belt is shown decidedly darker than the rest, as observed last year.—*Monthly Notices*, June 1873..

Zone c. The only feature noticed in this, the brightest zone of the planet, was a small projection from the Equatoreal zone, shown in sketches 12, 22, and 27.

Zone 3. The Equatoreal zone has exhibited many curious changes since the last opposition. The northern portion, 3 III., shows in some drawings a narrow horizontal bright band extending all across the disk; in others there are great breaks and gaps, which sometimes appeared to have no line separating them from c, but these gaps were always observed to be less bright than the adjacent zone. A curious dark spot was noticed on the north edge of 3 III. on April 23rd.

The Equator, 3 II., was on all occasions observed to be marked by a bright band, which contains some very curious bright spaces, referred to hereafter.

The South Tropical belt, 3 I., has appeared invariably darker than 3 III. This portion of the Equatoreal zone contains a bright horizontal division, which has not been noticed to be continuous all round the planet; only one sketch between longitudes 70° and 190° gives any indication of its existence between those meridians. In some drawings the northern edge of 3 I. is marked by a much darker line. A curious thinning out of this belt towards the east is observed about longitude 320°. Between longitude 64° and 152° this belt bulges out and widens towards the south by reason of a slant streak from 2, which apparently extends itself along the south edge of § I., and so widens it, but the extra width appeared rather fainter than the rest of 3 I.

Zones 2 and b. The region occupied by these two zones has displayed more evidence than any other of change since last year. In long. 330° a curious curved projection from 2 stretches downwards across b to the Equatoreal zone; this has been a permanent feature during the whole period of observation. East and west of this curved projection 2 seems entirely broken up into white cloudy patches and breaks in the continuity of the belt. These white patches have not been noticed between longitudes 40° and 160°. About longitude 40° this South Temperate belt 2 begins to send out to the north-east a slant streak, which continues until it joins the Equatoreal zone in longitude 70°. In some drawings of the planet between longitudes 100° and 270° a narrow dark belt is shown running along the centre of b.

Zone a. This bright band has very rarely been seen sharply defined, though it has been detected on most nights of observation. In sketch 31 a narrow dark streak is shown running along the centre of this zone.

Zone 1. The South Polar zone has always appeared fainter than the North.

The colours of *Jupiter* this year have been far more conspicuous than in 1873. A marked change in the tint of the Equatoreal zone has taken place. In May 1873 it was observed of a decided brick red tint; on no occasion this year has that tint been remarked, but a bronze yellow or sienna (mixture of raw and burnt sienna) has prevailed for the whole period of observation, though perhaps on one or two nights the tint approached more of a rich yellow.

The bright band c, immediately north of the Equatoreal zone, has appeared always blue, whether from contrast with the adjacent region or not I cannot say, as what observations I could make with the Equatoreal zone concealed by a bar were unsatisfactory.

One of the most noticeable features in the colours of *Jupiter* is the difference in tint of the North and South Polar zones, already recorded and depicted by Lord Rosse in the interesting series of chromolithographs published in the *Monthly Notices* for March 1874. The South Polar zone has seemed to partake of the tint of the Equatoreal zone in a fainter degree; while the North is, as it has appeared for the last few years, of a bluish grey.

In Mr. Browning's beautiful coloured drawing of *Jupiter*, published in *The Student* for 1869, the dark southern band of the Equatoreal zone is depicted and described as "madder-brown." This particular tint I observed in the same portion of the Equatoreal zone (the South Tropical belt) on several occasions, noticeably on March 31st, at 10.40, when the colours were compared with Mr. Browning's drawing.

I should add that distinct colour views of *Jupiter* seem to go hand in hand with sharp definition, and when the air has been unsteady, colours have been imperfectly seen. With regard to the power necessary to show colour, I must say that a Browning-achromatic eyepiece of 144 has revealed them to me as distinctly and decidedly as any higher power.

The darkest portion of the disk is undoubtedly the South Tropical belt, but this is not uniform in tint, as mentioned above.

The brightest spaces were observed on the Equator in the central light band of the Equatoreal zone. These it has been impossible to represent in the sketches. Particular bright (almost brilliant*) narrow streaks were observed adjacent to the dark South Tropical belt, 3 III. Nothing exactly corresponding is noticed in Lord Rosse's drawings, but somewhat similar light spaces are depicted by Mr. Browning in his drawing before referred to.

* Tacchini describes the appearance in 1873 of certain bright streaks on the Equator of *Jupiter*, as "très vives, comme argentées."

An excellent observation of Mr. Lassell's bright spots on the Southern belts was made on May 17th, at 10.0. I was examining the planet with 144, and made a sketch of it, definition being good. Thinking to improve definition, I put on a 6½-inch stop, and immediately perceived a brilliant spot like a minute satellite on the south edge of the Equatoreal zone, in zone b. I should say it appeared more brilliant than a satellite, and smaller. Further search enabled me to make out two more spots on the same belt. It is said that these spots are seen only with large apertures; but I have no doubt that reducing the aperture from 8½ inches to 6½ inches first revealed them to me, by materially reducing the brightness of the planet's disk, while these minute spots were brought out in greater contrast; hence they must be of considerable intrinsic brightness. On this evening, with the full aperture, the belts were seen more minutely than with the 6½-inch stop on, but the bright spots were not nearly so distinct as with the reduced aperture. This fact I satisfied myself about without doubt.

The shadows of the satellites on the disk have been inserted in the drawings as near as possible in the positions observed with regard to the belts. On March 25th the transits of II. and III. with their shadows were observed. At 8.30 III. appeared as a small dusky spot, not round but elongated E. and W., which traversed the edge of the North Polar zone, and a little north of its shadow, which made the transit along the centre of the bright division d. II. and its shadow transitted the North Temperate belt. At 10 o'clock, some minutes before the satellites had begun to protrude beyond the edge of the disk, they were both very distinctly visible as white spots, though when II. was near the centre I could see it only with the greatest difficulty. Precisely the same phenomenon was observed on April 26th, when II. was in transit, and on May 2nd, when Satellite I. was on the disk. On both occasions the satellites were nearly invisible in mid-transit, and on nearing the edge they started into distinct visibility.

The greater distinctness of the satellites themselves when near the limb is a curious phenomenon which has been repeatedly observed by astronomers, but which seems to require explanation. On the occasions mentioned above II. transitted a dark belt, which was most dark near the centre, and fainter towards the limb, yet the satellite was almost invisible when on the darkest part of the belt, and was bright and distinct when the background of the belt was faintest. Satellite III. on March 25th, 1874, appeared as a dark spot when in mid-transit, and on nearing the edge appeared as a bright spot without trace of duskiness. But on March 26th, 1873, Satellite IV. made the whole transit as a dark spot, and was not perceptibly less dark at egress than in mid-transit.

Cloudy weather has prevented any observation of IV. in transit this year. On May 2nd, at 6.40, when this satellite was

on the disk, the planet was picked up in daylight, and though all the belts could be discerned, I could detect nothing of the satellite.

Since writing the above, I have read Mr. Brett's paper on *Jupiter*, in the last number of the *Monthly Notices*, which states that he has detected a dark border upon the white patches on the Equatoreal zone; and this border being always situated on the side furthest from the Sun, he therefore argues that these white patches must be high above the body of the planet and cast shadows.

The feature to which Mr. Brett directs attention is a very curious one, which I do not think has altogether escaped my notice; though as no time is mentioned when this phenomenon was observed by him, I do not know to which particular spots he refers. In the accompanying drawings I have endeavoured to represent similar appearances, if not identically the same; but if so, it is difficult to make them accordant with Mr. Brett's theory.

Jupiter was in opposition to the Sun on March 17th. In sketch No. 14, made on March 24th, at eleven hours, a large white patch or break will be noticed in the northern portion of the Equatoreal zone. The east boundary of this patch was observed much darker than the west, and this darker portion extended for about 5° or 6° of Longitude. Again, in sketch No. 18, made on March 31st, at 10.40, only fourteen days after opposition, the same white patch is shown; and as the dark border east of it was carefully observed that night, I endeavoured to delineate, as accurately as I could, its apparent length (east and west), which I find to be about 8° or 10° of Longitude. Now, it would be easy to calculate what the height of the white patch should be to cast a shadow that length, only fourteen days after opposition. This particular white patch has been observed up to May 16th, and is depicted in two sketches made that evening. In these drawings the dark following space certainly gives a *primâ facie* support to the theory of shadows; but inasmuch as this dark space was observed no longer (east and west) on this evening—two months after opposition—than on March 31st, I must think a true solution of the appearance has hardly been found.

I should add that some of the white patches were not noticed to have any dark border, whilst others, such as those represented in sketches 17, 22 and 30, had dark boundaries on both sides.

Stapenhill, Burton-on-Trent,
1874, *June* 9.

Third Paper on the Probable Variability of some of the Red Stars in Schjellerup's List (Astr. Nachrichten, No. 1591).

By John Birmingham, Esq.

(*Communicated by E. Dunkin, Esq., Hon. Secretary.*)

In two former papers laid before the Society I noticed the probable variability of certain stars in the above Catalogue, viz.: —Nos. 63, 77, 90, 91, 101, 152, 241, and 280; and I would now direct attention to Nos. 43, 132, 136, 138, 145, 168, 169, 173, 186, 189, 194, 198, 205, 206, 218, 219, 222, and 247, in each of which I think I have detected evidence of change either in magnitude or colour, or in both. These stars will be generally convenient for observation between this and the next meeting of the Society; and then I hope to communicate my observations of others of the Catalogue that do not well agree with the descriptions transcribed by Schjellerup, but yet require a more extended investigation before one might properly suggest the question of their variability.

I may here state that I have generally determined the magnitudes by estimation, as I have found that, by considerable and careful practice, it is possible in that way to arrive at results which will well bear the test of comparison with the work of "limiting apertures." Between the magnitudes 6 and 10 I believe that I may be considered correct within the limits of a half-magnitude; and, while I feel that, under any circumstances, I could scarcely err to the extent of a whole magnitude, I would remark that this is generally exceeded by the changes which I attribute to the stars under notice.

In judging of colour I am persuaded that in many instances stars appear to me less red than they do to other people; and I should, therefore, be slow to conclude that there must, necessarily, be a change in stars which, though previously described as "ruby," or genuine red, appear to me with a decided tint of orange. However, where the difference between my estimates of colour and those of former observers is very great—such, for instance, as appears in the descriptions of Nos. 168, 173, &c.— the discrepancies must be considered something more than matters of mere personal equation. Nor do I think that differences so striking could be referred to instrumental causes. The refractor with which I have made my observations, though only of $4\frac{1}{2}$ inches aperture, seems very capable of showing the full colour of red stars, as I always find by the variable U *Cygni*, which appears of a deep crimson when even not exceeding the 10th magnitude.

In the following list the numbers refer to Schjellerup's Catalogue, from which I transcribe the descriptions and magnitudes, and then subjoin my own observations with the dates. The positions are for 1872:—

$$43.\quad \alpha = 4^{h}\ 43^{m}\ 30^{s} : \delta = +28^{\circ}\ 18'\ 13''.$$

Cape Observatory, Ruby-coloured. Intense and beautiful colour, 8.

Pale red, 8; 1872, February 10.—Orange red; 1873, January 22.—Good orange red, 7·5; 1874, February 9.—Good red, 7·5; February 15.—Good red, 7·5; February 17.—Good red; scarcely more than 8; February 18.—Colour as usual, 8; February 21.—Good red, 7·8; rather low; February 27.—Fine red, 8; March 6.—Fine red, 8; March 12.—Good red, 7; March 24.—Good red, 8·8-9; April 11.

$$132.\quad \alpha = 10^{h}\ 31^{m}\ 15^{s} : \delta = -12^{\circ}\ 43'\ 13''.$$

Hist. Cel. p. 329; *rouge. Bessel*; *roth. Lamont*; *orange*, 6·5.

Fine red with orange cast, 6-6·5; 1873, March 12.—Fine orange star, 5m. at least, and could probably be seen with naked eye only for mist and moonlight; 1874, February 27.—Splendid red orange, 6; March 24.—Fine red orange, 5; April 7.—Splendid red orange, glimpsed occasionally in bad atmosphere with naked eye; May 8.

$$136.\quad \alpha = 10^{h}\ 45^{m}\ 24^{s} : \delta = -20^{\circ}\ 34'\ 20''.$$

Hist. Cel. p. 284; *bien rouge. Arg.*; *sehr roth*, 6·5.

Fine red, less than 7; 1873, April 20.—Fine red, 8, invisible in finder; 1874, May 8.

$$138.\quad \alpha = 10^{h}\ 54^{m}\ 15^{s} : \delta = -17^{\circ}\ 38'\ 14''.$$

Cape Obs. Most intense and curious colour; *scarlet*, 8.

Very red, 8; 1872, April 13.—Good red, seems under 8; 1873, March 12.—Good red with orange cast, less than 8; April 20.—Genuine ruby, 9-10; 1874, April 7.

$$145.\quad \alpha = 12^{h}\ 18^{m}\ 41^{s} : \delta = +1^{\circ}\ 28'\ 44''.$$

Bessel; *roth. Lamont, Z* 93; *rubra. D'Arrest*; *rubra*, 7·5.

A very fine red star, 7; 1872, April 15.—Fine red, 6-6·5; 1874, February 27.—Fine deep orange red, 6-6·5, April 10 and May 8 and 9.

$$168.\quad \alpha = 14^{h}\ 18^{m}\ 0^{s} : \delta = +8^{\circ}\ 40'\ 14''.$$

D'Arrest; *fulva et quidem egregie*, 6.

No tinge of red; 1872, April 15 and 27.—A tinge of very palest orange; May 9.—Perhaps a slight yellow cast; 1873. March 14.—Certainly not red; light straw colour, 6·5; April 21, —No certain colour; April 23.—No striking colour; perhaps light straw colour, 7; 1874, April 10.—Rather reddish, contrasted with a blue between it and 96 *Boötis*, and smaller than the blue; May 7.—Rather reddish, 6-6·5; June 7.

$$169. \quad \alpha = 14^{h}\ 18^{m}\ 25^{s} : \delta = +26^{\circ}\ 17'\ 14''.$$

Cape Obs. vivid red; almost bright ruby, 7·5.

Fine red orange, 7·3; 1872, April 7.—Fine red, 9; 1874, April 10.—Good red, 7·5; June 7.

$$173. \quad \alpha = 14^{h}\ 29^{m}\ 25^{s} : \delta = +37^{\circ}\ 11'\ 21''.$$

Hist. Cel. p. 164; *rouge,* 6.

Faint yellow tinge, 1872, April 27.—Faint straw colour, *if any,* 7; 1874, April 10.—Faint straw colour, 6; May 9.—Faint straw colour, 6-6·5; June 7.

$$186. \quad \alpha = 16^{h}\ 3^{m}\ 11^{s} : \delta = +1^{\circ}\ 9'\ 40''.$$

D'Arrest; subrubra, 8.

Reddish, 7; 1872, April 27.—Pale reddish, 8; 1873, March 9.—Reddish, 7·5-8; 1874, April 10.—Reddish, 7; May 7.—Dull reddish tinge, 7-7·5; June 7.

$$189. \quad \alpha = 16^{h}\ 19^{m}\ 37^{s} : \delta = -12^{\circ}\ 7'\ 34''.$$

Cape Obs. dull brick red, 8.

Good red, 9-9·5; 1873, April 6.—Genuine ruby, 9; 1874, April 10.—Fine red, 7·5; May 8.

$$194. \quad \alpha = 16^{h}\ 44^{m}\ 35^{s} : \delta = -5^{\circ}\ 57'\ 19''.$$

Schj. 5991; *roth,* 8.

Not seen; 1872, May 9.—Not red, 9·5; 1873, April 6 and 20.—Seems less than 9, and pale reddish; April 23.—Perhaps reddish, 10-11; 1874, April 10.—Reddish, 10 at most; May 8 and 9.

$$198. \quad \alpha = 16^{h}\ 53^{m}\ 4^{s} : \delta = -4^{\circ}\ 1'\ 36''.$$

Schj. 6046; *gelbroth,* 8.

Slight reddish tinge [30 *Ophiuchi.* Near it is a fine amber-coloured star with orange cast] 1873, April 6.—Slightly, if at all, red, 9·5; 1874, April 10.—Slightly, if at all, reddish, 9; May 8.

$$205. \quad \alpha = 17^{h}\ 37^{m}\ 25^{s} : \delta = -18^{\circ}\ 35'\ 51''.$$

Cape Obs. Remarkable ruby red, 8.

Slightly red, not more than 9; 1873, May 18.—Red not deep, 9-9·5; 1874, April 10.—Reddish, 8-9; May 7.

$$206. \quad \alpha = 17^{h}\ 47^{m}\ 23^{s} : \delta = +1^{\circ}\ 47'\ 39''.$$

Cape Obs. A curious ruby-coloured star, 9.

Tinged with red, 7-8; 1871, September 4.—Not seen; 1872,

May 9.—Not seen; 1873, April 20.—Not seen (strong twilight); 1874, April 10.—Not seen (fine sky); May 8.—The Rev. Mr. Webb has independently remarked the variability of this star.

218. $\alpha = 18^{h}\ 39^{m}\ 34^{s} : \delta = +8^{\circ}\ 37'\ 1''$.

Cape Obs. Plum-coloured, or ruddy purple, 9.

Not seen; 1872, May 11.—Not seen; 1874, May 8.

219. $\alpha = 18^{h}\ 42^{m}\ 52^{s} : \delta = -8^{\circ}\ 2'\ 58''$.

Cape Obs. Most remarkable ruby red, 9.

Fine orange red, 7·5-8; 1873, April 18.—Very fine orange red, 7-8; September 23.—Fine orange red, 8 at least; 1874, May 8.

222. $\alpha = 18^{h}\ 52^{m}\ 42^{s} : \delta = +14^{\circ}\ 11'\ 9''$.

Lamont, Z 199; *rubra*, 8.

Fine red, 8; 1872, May 16.—A good red, 9-9·5; 1873, April 6.—Fine red, 9-5.10; 1874, May 8. Secchi says of this star in his "Memoria II.," presented to the Italian Society in November 1868: "È piccola di 9ª, e nulla ha di particolare nello spettro." He gives no date of observation.

247. $\alpha = 21^{h}\ 9^{m}\ 34^{s} : \delta = +59^{\circ}\ 35'\ 12''$.

Conn. d. T. xv.; *rouge. Arg.*; *sehr roth*, 8.

Red, not deep, 6·5; 1872, July 14.—Orange red, 6·5; 1873, September 28. Secchi, in the Memoir already quoted, says of this star: "Debolmente rossa di 8, 9, senza nulla di singolare nello spettro."

Phenomena of Jupiter's Satellites observed at the Stonyhurst Observatory in 1873–74. By the Rev. S. J. Perry.

Date. 1873.	Satellite.	Phenomenon.	G.M.T. h m s	Observer.	Remarks.
May 2	II.	Sh. I. internal contact	8 58 18·3	S. P.	Probably late.
		Tr. E. int. contact	9 9 56·3	,,	
		bisection	9 14 12·8	,,	

Date.	Satellite.	Phenomenon.	G.M.T. h m s	Observer.	Remarks.
1873.					
May 9		Tr. I. external contact	8 54 27·5	W. C.	
		bisection	8 57 6·5	„	
		int. contact	8 59 52·0	„	Thin clouds.
May 11		Ecc. R. first seen	9 30 1·6	S. P.	Very good.
		full brightness	9 32 26·8	„	
May 12	I.	Occ. D. last seen	11 48 28·4	„	Clouds passing.
May 13		Tr. I. ext. contact	9 7 58·2	„	Slight mist. Definition not good.
		bisection	9 10 27·5	„	
		int. contact	9 12 42·5	„	
		Sh. I. bisection	10 28 40·5	„	Probably late.
		int. contact	10 30 14·0	„	
		Tr. E. int. contact	11 25 40·2	„	Mist, very bad.
		bisection	11 28 51·5	„	
		ext. contact	11 31 33·0	„	
May 14		Ecc. R. first seen	9 51 12·6	„	Slight haze.
May 21	III.	Sh. E. bisection	8 47 51·3	W. C.	Very good. Shadow very dark.
		last seen	8 49 39·8	„	
May 22	I.	Sh. E. int. contact	9 3 53·1	„	Faint. Boiling.
		bisection	9 5 19·6	„	
		last seen	9 7 2·6	„	
	IV.	Ecc. R. first seen	10 44 12·6	„	
May 28	III.	Sh. I. bisection	9 24 1·6	S. P.	
		int. contact	9 26 47·1	„	
	I.	Occ. D. first contact	10 13 9·0	„	
		bisection	10 15 27·1	„	
June 4	III.	Tr. I. ext. contact	8 21 33·1	„	
		bisection	8 25 10·6	„	Daylight.
		int. contact	8 28 18·6	„	
Oct. 19	III.	Sh. I. bisection	16 53 38·6	W. C.	Not very good.
Oct. 28	I.	Ecc. D. last seen	16 30 21·9	„	
Nov. 12	I.	Tr. I. ext. contact	18 17 27·5	„	
		bisection	18 20 2·0	„	
		int. contact	18 22 51·5	„	
Nov. 25	IV.	Ecc. R. first seen	16 2 13·1	„	
Dec. 11	III.	Occ. D. first cont.	15 47 36·0	„	
		bisection	15 51 22·5	„	
		last seen	15 54 20·0	„	
1874.					
Feb. 3	III.	Tr. E. bisection	10 53 58·1	„	Clouds passing.
		ext. contact	11 0 21·1	„	

Date. 1874.	Satellite.	Phenomenon.	G.M.T. h m s	Observer.	Remarks.
Feb. 3	II.	Sh. I. int. contact	14 22 30·1	W.C.	Very faint.
		Tr. I. first contact	15 53 17·1	„	
		bisection	15 56 18·6	„	
		int. contact	15 59 34·1	„	
		Sh. E. int. contact	16 43 35·1	„	Shadow very dark.
		bisection	16 46 18·1	„	
		last seen	16 48 55·1	„	
		Tr. E. int. contact	18 22 5·1	„	
		bisection	18 28 8·1	„	Tremulous. Thin clouds.
		ext. contact	18 33 44·1	„	Tremulous. Thin clouds.
	I.	Ecc. D. light fading	18 50 20·1	„	
		last seen	18 53 46·1	„	
Mar. 9	II.	Ecc. D. last seen	8 14 37·5	„	
Mar. 18	III.	Tr. E. bisection	7 1 11·1	„	
		last contact	7 6 13·1	„	
	II.	Tr. E. bisection	8 3 7·1	„	
		ext. contact	8 8 20·1	„	
	I.	Occ. R. bisection	8 27 58·1	„	
		last contact	8 31 39·1	„	
Mar. 25	II.	Tr. I. bisection	7 40 24·68	„	
		last contact	7 44 35·2	„	
	III.	Sh. E. first contact	11 2 18·2	„	
		bisection	11 6 20·7	„	
		last contact	11 10 30·7	„	
May 17	I.	Ecc. R. first seen	12 37 4·1	„	
	II.	Tr. I. ext. contact	12 4 54·6	„	
		bisection	12 59 44·5	„	
		int. contact	13 4 45·6	„	

The above observations were made during the twelve months from May 1873 to May 1874. Most of the observations in 1873, and all in 1874, were taken by Mr. W. Carlisle. The instrument used was the 8-inch Equatoreal.

Stonyhurst Observatory,
1874, *June* 12.

Observations of Occultations of Stars by the Moon, 1873 and 1874, (with the deduced Equations between the Errors of the Lunar Elements); and of Phenomena of Jupiter's Satellites in 1874: made at the Radcliffe Observatory, Oxford.

(Communicated by the Radcliffe Observer.)

Occultations.

No.	Day of Observation.	Phenomenon.	Moon's Limb.	Oxford Mean Solar Time.	Ob-server.
	1873.			h m s	
1	July 4	Dis. of λ Virginis	Dark	10 9 9·3	M & K
2	Oct. 3	„ τ^1 Aquarii	„	6 26 22·5	K
3	Nov. 5	„ A^1 Tauri	Bright	11 56 57·1	L
4	Dec. 24	„ τ^2 Aquarii	Dark	4 24 34·3	M & K
	1874.				
5	Jan. 25	Dis. of 53 Arietis	Dark	11 43 46·9	K
6	„ 26	„ A^1 Tauri	„	12 30 20·9	L
7	„ 27	„ *k* Tauri	„	10 34 13·0	K
8	„ 30	„ *c* Geminorum	„	6 0 16·1	K
9	Mar. 26	„ λ Cancri	„	11 41 34·6	K
10	Apr. 22	„ ω^1 Cancri	„	9 39 35·7	K
11	„	„ ω^2 Cancri	„	10 6 20·0	M & K

Notes.

1873. July 4. The disappearance was instantaneous and the observation good. The mean of the observed seconds 9·0 and 9·5 has been taken.
Oct. 3. The observation good.
Nov. 5. The Moon and star rather unsteady; thin cloud.
Dec. 24. Very exact (M.). Very good (K.). The mean of the observed seconds 34·5 and 34·2 has been taken.
1874. Jan. 25, Jan. 26, Jan. 27, Jan. 30, Mar. 26. The disappearances were instantaneous and the observations good.
Apr. 22. (Disapp. of ω^2 *Cancri.*) Very good (M.). The mean of the observed seconds 19·8 and 20·2 has been taken.
„ (Disapp. of ω^1 and ω^2 *Cancri.*) Disappearances instantaneous and the observations good (K.).

In the following table of the errors of lunar elements resulting from the occultations the Greenwich notation is used, and the elements of the *Nautical Almanac* are used uncorrected. All the computations have been made by Mr. Main by the method given in his treatise on *Spherical and Practical Astronomy*.

The observations are referred to by the Nos. of reference given above.

$$1,\ +6''\!\cdot\!42 = +0\cdot378 \times e + 0\cdot922 \times f - 0\cdot378 \times x - 0\cdot923 \times y - 0\cdot320 \times t - 2\cdot144 \times m - 0\cdot916 \times n.$$

2, $+9''.48 = +0.960 \times e - 0.151 \times f - 0.960 \times x + 0.150 \times y - 0.494 \times t$
$- 1.272 \times m - 0.996 \times n.$

3, $+4.38 = +0.146 \times e - 0.988 \times f - 0.146 \times x + 0.988 \times y - 0.248 \times t$
$+ 1.675 \times m - 0.967 \times n.$

4, $+8.97 = +0.973 \times e + 0.016 \times f - 0.973 \times x - 0.018 \times y - 0.374 \times t$
$- 0.104 \times m - 0.971 \times n.$

5, $+8.73 = +0.578 \times e - 0.794 \times f - 0.578 \times x + 0.794 \times y - 0.427 \times t$
$+ 3.223 \times m - 0.954 \times n.$

6, $+8.47 = +0.924 \times e + 0.052 \times f - 0.924 \times x - 0.051 \times y - 0.491 \times t$
$+ 1.948 \times m - 0.945 \times n.$

7, $+8.77 = +0.899 \times e - 0.104 \times f - 0.899 \times x + 0.107 \times y - 0.416 \times t$
$+ 1.328 \times m - 0.937 \times n.$

8, $+5.50 = +0.657 \times e - 0.678 \times f - 0.657 \times x + 0.679 \times y - 0.353 \times t$
$- 0.045 \times m - 0.915 \times n.$

9, $+6.15 = +0.367 \times e + 0.914 \times f - 0.367 \times x - 0.914 \times y - 0.319 \times t$
$- 1.035 \times m - 0.908 \times n.$

10, $+7.83 = +0.825 \times e - 0.392 \times f - 0.825 \times x + 0.394 \times y - 0.345 \times t$
$+ 2.361 \times m - 0.922 \times n.$

11, $+6.30 = +0.766 \times e + 0.524 \times f - 0.766 \times x - 0.522 \times y - 0.469 \times t$
$+ 0.562 \times m - 0.922 \times n.$

Phenomena of Jupiter's Satellites.

Day of Obs.	Satellite.	Phenomena.	Instrument and Power used.	Oxford Mean Solar Time of Observation.	Greenwich Mean Solar Time from N. A.	Observer.
1874.				h m s	h m s	
Jan. 30.	I.	Tr. egr. bisection	Heliom.	11 45 4·6	11 51	B
		„ last contact	with power 150	11 46 49·3		
	IV.	Ecl. disappearance	„	12 23 23·6	12 32 10·0	K
Mar. 7.	III.	Ecl. disappearance	„	10 23 24·0	10 24 23·0	„
„ 9.	II.	Occ. reap. first app.	10-ft. tel.	11 7 32·4	11 14	„
		„ last contact	with power 150	11 12 31·6		
	I.	Occ. reap. bisection	„	12 12 51·9	12 18	„
		„ last contact	„	12 14 51·6		
„ 18.	II.	Tr. egr. first contact	Heliom.	7 55 59·0	8 4	„
		„ bisection	with power 200	7 57 58·7		
		„ last contact	„	8 0 28·2		

Day of Obs.	Satellite.	Phenomena.	Instrument and Power used.	Oxford Mean Solar Time of Observation. h m s	Greenwich Mean Solar Time from N.A. h m s	Observer.
1874.						
	I.	Occ. reap. first app.	Heliom.	8 20 54·8		
		„ bisection	with power	8 22 24·6	8 28	K
		„ last contact	200	8 24 24·3		
Mar. 24.	I.	Tr. ingr. first contact	10-ft. tel.	10 34 1·3	10 43	L
		„ last contact	with power 150	10 39 10·4		
		„ first contact	Heliom.	10 33 8·2		
		„ bisection	with power 200	10 35 17·8	10 43	B
		„ last contact	„	10 38 47·2		
„ 25.	III.	Tr. ingr. first contact	„	7 11 25·7		
		„ bisection	„	7 14 55·1	7 21	K
		„ last contact	„	7 19 24·5		
	II.	„ first contact	„	7 34 22·0		
		„ bisection	„	7 37 21·5	7 41	„
		„ last contact	„	7 40 36·0		
	I.	Occ. disap. first contact	„	7 48 49·6		
		„ bisection	„	7 50 49·3	7 56	„
		„ last contact	„	7 52 49·0		
	II.	Tr. egr. first contact	„	10 7 56·8		
		„ bisection	„	10 10 26·4	10 17	„
		„ last contact	„	10 13 25·9		
	III.	Tr. egr. first contact	„	10 7 56·9		
		„ bisection	„	10 11 56·2	10 22	„
		„ last contact	„	10 15 55·5		
	I.	Ecl. reap. first seen	„	10 16 10·5	10 21 2·5	„
		„ full brightness	„	10 19 24·9		
	II.	Sh. egr. first contact	„	10 30 23·1	10 43	„
	III.	Sh. egr. first contact	„	11 1 48·0		
		„ bisection	„	11 5 47·3	11 15	„
		„ last contact	„	11 8 46·8		
„ 26.	I.	Tr. egr. first contact	„	7 16 28·6		
		„ bisection	„	7 18 28·3	7 24	„
		„ last contact	„	7 20 28·0		
„ 31.	I.	Tr. ingr. first contact	10-ft. tel.	12 20 25·0		
		„ bisection	with power 150	12 22 24·7	12 27	L
		„ last contact	„	12 25 4·3		

Day of Obs. 1874.	Satellite.	Phenomena.	Instrument and Power used.	Oxford Mean Solar Time of Observation. h m s	Greenwich Mean Solar Time from N.A. h m s	Observer.
	I.	Sh. ingr. last contact	10-ft. tel.	12 47 0·7	12 47	L
Apr. 8.	I.	Occ. dis. first contact	with power 150	11 17 14·0		
		„ bisection		11 19 23·7	11 24	„
		„ last contact	„	11 20 43·5		
	II.	Tr. ingr. first contact	„	12 2 13·1		
		„ bisection	„	12 5 12·6	12 8	„
		„ last contact	„	12 9 31·9		
„ 10.	I.	Ecl. reappearance	„	8 32 20·2	8 37 26·0	„
	II.	Ecl. reap. first seen	„	10 30 2·3	10 36 15·4	„
		„ fully seen	„	10 30 22·3		
	I.	Ecl. reappearance	Heliom.	8 32 20·2	8 37 26·0	B
	II.	Ecl. reappearance	with power 200	10 30 2·3	10 36 15·4	„
„ 24.	I.	Occ. dis. first contact	10-ft. tel.	9 14 51·2	9 22	L
		„ last contact	with power 150	9 19 30·4		
	II.	Occ. dis. first contact	„	11 21 18·9	11 30	„
		„ last contact	„	11 25 58·1		
„ 25.	I.	Tr. egr. first contact	Heliom.	8 43 36·3		
		„ bisection	with power 200	8 45 36·0	8 55	K
		„ last contact	„	8 48 35·5		
May 2.	I.	Tr. ingr. first contact	„	8 19 42·4		
		„ bisection	„	8 22 12·0	8 26	B
		„ last contact	„	8 25 41·4		
	I.	Sh. ingr. first seen	„	9 22 47·1	9 24	K
	I.	Tr. egr. bisection	„	10 39 19·5	10 43	„
		„ last contact	„	10 41 19·2		
„ 7.	III.	Sh. egr. last seen	10-ft. tel.	10 57 37·0	11 5	„
„ 12.	II.	Ecl. reap. first seen	with power 150	10 15 34·9	10 21 54·5	L
		„ fully seen	„	10 16 12·8		
„ 16.	I.	Tr. ingr. first contact	Heliom.	12 0 6·1		
		„ bisection	with power 200	12 2 35·7	12 6	K
		„ last contact	„	12 5 35·2		
„ 18.	I.	Tr. egr. first contact	„	8 43 16·8		
		„ bisection	„	8 45 16·5	8 51	„
		„ last contact	„	8 47 16·2		

Day of Obs.	Satellite.	Phenomena.	Instrument and Power used.	Oxford Mean Solar Time of Observation.	Greenwich Mean Solar Time from N.A.	Observer.
1874.				h m s	h m s	
	I.	Occ. reap. first app.	Heliom.	8 20 54·8		
		„ bisection	with power	8 22 24·6	8 28	K
		„ last contact	200	8 24 24·3		
Mar. 24.	I.	Tr. ingr. first contact	10-ft. tel.	10 34 1·3		
		„ last contact	with power 150	10 39 10·4	10 43	L
		„ first contact	Heliom.	10 33 8·2		
		„ bisection	with power 200	10 35 17·8	10 43	B
		„ last contact	„	10 38 47·2		
„ 25.	III.	Tr. ingr. first contact	„	7 11 25·7		
		„ bisection	„	7 14 55·1	7 21	K
		„ last contact	„	7 19 24·5		
	II.	„ first contact	„	7 34 22·0		
		„ bisection	„	7 37 21·5	7 41	„
		„ last contact	„	7 40 36·0		
	I.	Occ. disap. first contact	„	7 48 49·6		
		„ bisection	„	7 50 49·3	7 56	„
		„ last contact	„	7 52 49·0		
	II.	Tr. egr. first contact	„	10 7 56·8		
		„ bisection	„	10 10 26·4	10 17	„
		„ last contact	„	10 13 25·9		
	III.	Tr. egr. first contact	„	10 7 56·9		
		„ bisection	„	10 11 56·2	10 22	„
		„ last contact	„	10 15 55·5		
	I.	Ecl. reap. first seen	„	10 16 10·5		
		„ full brightness	„	10 19 24·9	10 21 2·5	„
	II.	Sh. egr. first contact	„	10 30 23·1	10 43	„
	III.	Sh. egr. first contact	„	11 1 48·0		
		„ bisection	„	11 5 47·3	11 15	„
		„ last contact	„	11 8 46·8		
„ 26.	I.	Tr. egr. first contact	„	7 16 28·6		
		„ bisection	„	7 18 28·3	7 24	„
		„ last contact	„	7 20 28·0		
„ 31.	I.	Tr. ingr. first contact	10-ft. tel.	12 20 25·0		
		„ bisection	with power 150	12 22 24·7	12 27	L
		„ last contact	„	12 25 4·3		

Day of Obs. 1874.	Satellite.	Phenomena.	Instrument and Power used.	Oxford Mean Solar Time of Observation. h m s	Greenwich Mean Solar Time from N.A. h m s	Observer.
	I.	Sh. ingr. last contact	10-ft. tel.	12 47 0·7	12 47	L
Apr. 8.	I.	Occ. dis. first contact	with power 150	11 17 14·0		
		„ bisection		11 19 23·7	11 24	„
		„ last contact	„	11 20 43·5		
	II.	Tr. ingr. first contact	„	12 2 13·1		
		„ bisection	„	12 5 12·6	12 8	„
		„ last contact	„	12 9 31·9		
„ 10.	I.	Ecl. reappearance	„	8 32 20·2	8 37 26·0	„
	II.	Ecl. reap. first seen	„	10 30 2·3	10 36 15·4	„
		„ fully seen	„	10 30 22·3		
	I.	Ecl. reappearance	Heliom.	8 32 20·2	8 37 26·0	B
	II.	Ecl. reappearance	with power 200	10 30 2·3	10 36 15·4	„
„ 24.	I.	Occ. dis. first contact	10-ft. tel.	9 14 51·2	9 22	L
		„ last contact	with power 150	9 19 30·4		
	II.	Occ. dis. first contact	„	11 21 18·9	11 30	„
		„ last contact	„	11 25 58·1		
„ 25.	I.	Tr. egr. first contact	Heliom.	8 43 36·3		
		„ bisection	with power 200	8 45 36·0	8 55	K
		„ last contact	„	8 48 35·5		
May 2.	I.	Tr. ingr. first contact	„	8 19 42·4		
		„ bisection	„	8 22 12·0	8 26	B
		„ last contact	„	8 25 41·4		
	I.	Sh. ingr. first seen	„	9 22 47·1	9 24	K
	I.	Tr. egr. bisection	„	10 39 19·5	10 43	„
		„ last contact	„	10 41 19·2		
„ 7.	III.	Sh. egr. last seen	10-ft. tel.	10 57 37·0	11 5	„
„ 12.	II.	Ecl. reap. first seen	with power 150	10 15 34·9	10 21 54·5	L
		„ fully seen	„	10 16 12·8		
„ 16.	I.	Tr. ingr. first contact	Heliom.	12 0 6·1		
		„ bisection	with power 200	12 2 35·7	12 6	K
		„ last contact	„	12 5 35·2		
„ 18.	I.	Tr. egr. first contact	„	8 43 16·8		
		„ bisection	„	8 45 16·5	8 51	„
		„ last contact	„	8 47 16·2		

Day of Obs.	Satellite.	Phenomena.	Instrument and Power used.	Oxford Mean Solar Time of Observation. h m s	Greenwich Mean Solar Time from N.A. h m s	Observer.
1874.						
June 1.	III.	Ecl. dis. Began to fade in brightness	10-ft. tel.	10 3 6·0	10 9 47·3	K
		„ final dis.	with power 150	10 8 25·1		
	I.	Tr. ingr. first contact	„	10 10 4·9	10 18	„
		„ bisection	„	10 13 4·4		
		„ last contact	„	10 16 3·9		
„ 4.	II.	Tr. egr. first contact	Heliom.	9 52 55·9	9 56	„
		„ bisection	with power 200	9 55 25·5		
		„ last contact	„	9 57 55·1		
„ 8.	III.	Occ. dis. first contact	10-ft. tel.	8 43 14·6	8 50	„
		„ bisection	with power 150	8 47 13·9		
		„ last contact	„	8 51 13·2		
„ 9.	I.	Occ. dis. first contact	„	9 13 35·1	9 21	L
		„ last contact	„	9 17 24·5		
„ 11.	II.	Tr. egr. first contact	„	9 42 44·7	9 48	K
		„ bisection	„	9 45 44·2		
		„ last contact	„	9 49 43·5		
„ 13.	II.	Ecl. reap. first seen	Heliom. with power 200	10 1 54·0	10 7 36·3	„
		„ full brightness		10 4 8·6		

Notes.

Jan. 30. J. IV. Ecl. disapp. *Jupiter* and the satellites were very well defined. The satellite faded in brightness to a very small point of light, and disappeared suddenly at the time noted.

Mar. 7. The observation difficult, as the satellite disappeared close to the planet, and, before disappearing, became an extremely faint point of light.

„ 9. J. II. Occ. reapp. Doubtful; cloudy. J. I. Occ. reapp. The observation good.

„ 24. The planet was well defined and steady. (B.)

„ 25. J. II. and III. Trs. egress, and J. I. Ecl. reapp. The planet very tremulous.

„ 26. The planet very tremulous.

Apr. 24. Cloudy at both phenomena.

„ 25. The planet tremulous.

May 2. J. I. Trs. ingress. The planet very tremulous. J. I. Sh. Ingress. The shadow was well on the disc when "first seen."

„ 7. The sky became cloudy immediately after the time noted for "last seen."

„ 16. The image of the planet bad.

June 1. J. III. Ecl. disapp. The satellite became the merest point of light before finally disappearing.

„ 4. The image of planet bad.

9. The planet and satellite unsteady.
11. The planet tremulous.

The initials M., L., K., B., are those of Mr. Main, Mr. Lucas, Mr. Keating, and Mr. Bellamy.

The instruments used were the Heliometer and the 10-foot telescope.

The assumed longitude of the Radcliffe Observatory is $5^{m}\ 2^{s}{\cdot}6$ West.

Occultations of Stars by the Moon, and Eclipses of Jupiter's Satellites, observed at Windsor, New South Wales. By John Tebbutt, Esq.

The following is a list of lunar occultations of stars and eclipses of *Jupiter's* satellites observed by me during the year 1873. The occultations are disappearances only at the Moon's dark limb, the phenomena not being previously computed. The quadrant of the moon's limb, where it could be determined, at which each disappearance took place, will be found in the fourth column of the occultation table. Where the quadrant is not stated, it is to be understood, except in the case of May 12th, that the disappearances occurred in or very near the plane passing through the observer and the centres of the Sun and Moon. The second occultation for April 1st was observed with the old 3½-inch refractor, and a power of about 30; all the other occultations were observed with the 4½-inch Equatoreal, and a power of 55. The latter instrument, with a power of 180, was employed in all the eclipse observations.

Lunar Occultations of Stars, 1873.

Date of Obs.	Mag. of Star.	Observatory Mean Time of Disappearance.	Quadrant.	Remarks.
1874.		h m s		
April 1	4	6 53 44·3	S.	Disappearance seen through light cloud.
„ 1	7	7 23 52·5	S.	Star very faint on limb.
„ 5	8	8 9 32·0	...	Good observation. Disappearance pretty sudden.
„ 5	7	8 17 16·4	...	Very good observation. Disappearance sudden.
„ 5	8	8 20 32·4	S.	Pretty good obs., but uncertain, perhaps to half a second, owing to star's faintness on limb.
May 12	8	8 23 51·5	Moon totally eclipsed.	Disappearance pretty sudden.
„ 12	8½	8 35 45·3		Very faint at disappearance.
„ 12	8	8 37 5·8		Star appeared to advance slightly within limb.
„ 12	8½	8 53 59·3		
„ 12	9	9 11 54·3		Approximate, star being extremely faint.

Date of Obs.	Mag. of Star.	Observatory Mean Time of Disappearance.	Quadrant.	Remarks.
1873.		h m s		
June 2	7½	9 13 24·6	N.	Uncertain to half a second.
„ 2	7½	9 22 48·5	S.	Very good observation.
„ 3	7½	6 34 7·3	N.	
„ 5	7	10 8 8·4	N.	Disappearance pretty sudden.
„ 5	7	10 16 38·4	S.	Ditto ditto.
„ 8	5	9 2 4·5	S.	Very good observation. The star was distinct, and disappeared apparently by two stages.
July 31	7½	6 30 43·3	S.	Disappearance instantaneous.
Aug. 27	7	7 26 38·6	N.	Disappearance pretty sudden.
„ 28	8	6 51 27·0	N.	Star very faint. Obs. uncertain to 1 second.
„ 28	7½	7 1 24·1	...	Good obs. Disapp. not quite instantaneous.
„ 28	7	7 50 6·4	S.	Obs. very good. Disappearance instantaneous.
„ 28	7½	7 50 41·9	S.	Ditto ditto.
„ 28	8	8 24 12·9	S.	Ditto ditto.
„ 29	7½	6 32 19·7	N.	Good observation. ditto.
„ 29	7½	6 44 58·3	...	Good obs. Disappearance not quite instantaneous.
„ 29	7½	7 1 31·0	N.	Star faint. Uncertain to a quarter of a second.
„ 29	7½	9 27 53·0	...	Good obs. Disappearance sudden.
„ 29	7½	9 44 21·9	S.	Ditto. ditto.
„ 30	7	7 1 21·7	N.	Obs. very good. Disapp. not quite instantaneous.
„ 30	7	8 11 42·9	N.	Ditto. Disappearance instantaneous.
„ 30	7	8 42 33·9	N.	Ditto. ditto.
Sept. 2	2	13 27 17·2	N.	Disapp. instantaneous, but obs. uncertain to a considerable fraction of a second.
„ 30	7	8 48 18·4	N.	
Oct. 27	7	7 38 56·1	...	
„ 27	8	7 40 27·7	...	Star faint.
„ 27	7	8 34 53·6	N.	
„ 27	8	9 1 57·5	N.	
„ 28	7½	8 39 57·1	S.	Very good obs. Disappearance instantaneous.

Eclipses of Jupiter's Satellites, 1873.

Date of Obs. 1873.	Satellite.	Phase of Eclipse.	Observatory Mean Time of Phase.	Remarks.
			h m s	
April 4	IV.	D	10 17 14·0	Tolerable definition, but disapp. very gradual.
„ 6	II.	R	6 32 25·5	Sky beautifully clear. Good observation.
„ 21	IV.	R	8 52 41·3	Obs. late, owing to a passing cloud.

Date of Obs.	Satel-lite.	Phase of Eclipse.	Observatory Mean Time of Phase.	Remarks.
1874.			h m s	
Apr. 25	I.	R	8 37 47·3	Cloudless evening. Excellent definition.
May 11	I.	R	6 56 57·6	Moon near opposition. Observation through thin filmy clouds.
„ 15	II.	R	8 50 23·4	Good definition. Bright moonlight.
„ 18	I.	R	8 51 52·1	Sky slightly hazy. Pretty good definition.
„ 27	I.	R	5 15 47·7	Twilight. Beautifully clear and definition pretty good.
June 3	I.	R	7 11 0·8	Good definition, and sky beautifully clear. Moon in first quarter, and not far from planet.
July 12	I.	R	5 44 4·1	Sky clear, but planet boiling and somewhat tremulous. Belts pretty distinct.

Windsor, New South Wales, 1874, *April* 9.

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*Observations of the Zodiacal Light.**

By Vincent Fasel, Esq.

I would beg to submit to the Astronomical Society the following report on the Zodiacal Light, which, in the early months of the present year, has been displayed in this locality (Morges, Switzerland) with no ordinary brilliancy, and for that reason I feel the more induced to transmit my own observations, with the hope that they may prove interesting, and perhaps be of some value for comparison with those recorded in other distant places.

The first opportunity I had of examining the luminous phenomenon was on the evening of the 5th of February last, at 8^h p.m., L.M.T., when it exhibited the usual figure of a slightly inclined cone, with a very faint vertex, reaching, on this occasion, to a line drawn from θ *Arietis* to ξ^2 *Ceti*. The light was very distinct and bright, specially on the axis and towards the horizon, but the outline could not easily be made out. A line drawn from θ to γ *Arietis*, passing near to and to the south of ρ *Piscium* and γ *Pegasi*, down to the horizon, would indicate the northern boundary; the southern one, though not well defined, the light fading off gradually near it, passed from a little south of ξ^2 *Ceti*, taking in α *Piscium*, to about ϕ^1 *Ceti*. Sky clear, a fine starry night, and *Mars* shining brightly in a mass of diffused light. February 6th, at 7^h 15^m p.m., another fine clear evening; the Zodiacal Light more conspicuous than on the previous day, with a better defined conical figure. The contour of the vertex, extremely faint, could but just be traced out to π and μ *Arietis*. The light appeared quite as bright as the *Milky Way*, and with a

* The Paper was accompanied by two Drawings.—Ed.

greater intensity nearer the horizon and about *Mars*, which was nearly situated on the axis. At $7^h\ 20^m$ just observed a meteor crossing *Aries* from North to South; it was of the second magnitude, and of a red colour.

February 7th, at $7^h\ 18^m$ p.m., the Zodiacal Light very distinct, but not so brilliant in the vicinity of the vertex, whilst nearer the horizon it was remarkably luminous, and appeared to have a broader base—*quite* 30°; *Algenib* within the northern boundary, and 37 *Ceti* just outside the southern. Sky hazy and air calm.

February 9th, at 7^h p.m., the Zodiacal Light unusually brilliant. The smaller stars within it were scarcely visible, and the larger ones, with *Mars* itself, appeared evidently dimmed by its intensity. Yet, notwithstanding that great degree of luminosity, the curvature of the vertex was not easy to trace out. A drawing of the appearance was made on the spot, which will speak for itself, and where it will be seen that the axis lay *close by* and *parallel to* the ecliptic. The light was yellowish white, surpassing in vividness that portion of the *Milky Way* that runs through *Cassiopeia*, which was well situated for comparison. The air very clear, and sky cloudless, except over the distant Alps, a few clouds just crowning the summit of Mont Blanc.

February 10th, at $7^h\ 14^m$ p.m., again a vivid appearance of the phenomenon, the conical figure well defined, and *Mars* still situated nearly on the axis. The extent of the light and general features pretty much the same. The air remarkably clear, and the '*bise*' * rather sharp. In the early part of the day snow fell.

February 11th, at $7^h\ 10^m$ p.m., the Zodiacal Light again conspicuous, but not so bright nor so well defined as on the previous evening. The vertex hardly perceptible, extending only to a line drawn from ρ to σ *Piscium*. While gazing at it a fine meteor of the first magnitude, of the same colour as *Mars*, and with a short train, shot from East to West, within the zodiacal gleam, running with moderate velocity nearly parallel to the axis; passed close by *Mars*, the latter dividing its course into two equal parts. The extent of its path was estimated to be equal to a line drawn from *Algenib* to *Markab*. The effect was very pretty.

Between this and the 5th of next month an interval of cloudy or moonlight nights followed, during which one could now and then catch glimpses of the luminous gleam.

March 5th, at $7^h\ 30^m$ p.m., a very favourable view of the Zodiacal Light was obtained, the atmosphere being remarkably transparent and the sky perfectly cloudless. The northern boundary passed through γ *Arietis*, south of ρ *Piscium*, and just close to γ *Pegasi*; the southern, from ξ^2 *Ceti*, passed between α and ξ *Piscium*, directly to the horizon. The vertex reached δ *Arietis*. The light appeared considerably brighter than the *Milky Way*. *Mars* was a little to the south of the axis, between γ and ϵ *Piscium*. At $7^h\ 40^m$ a small meteor crossed *Perseus* from North to South.

* North wind.

March 6th, at $7^h\ 35^m$ p.m., sky very hazy and clouds round the western horizon. I could but just catch a glimpse of the upper part of the cone.

March 7th, at $7^h\ 50^m$ p.m., the southern boundary marked by *α Piscium*, and ξ^2 *Arietis*. The vertex very faint, and appeared to reach ζ *Arietis*.

March 8th, at $8^h\ 30^m$ p.m., the light decidedly more brilliant than on the two preceding nights. The northern boundary was boldly marked between γ and β *Arietis*, and the vertex extended nearly to the *Pleiades*.

March 10th, at 8^h p.m., as the twilight disappeared the Zodiacal Light became discernible, presenting the same features as before with regard to the extent and luminosity. The vertex appeared to involve τ^1 and τ^2 *Arietis*. *Mars* about the middle of the cone, and somewhat to the left of the axis.

March 14th, at $7^h\ 45^m$ p.m., an exceptionally fine evening, the sky perfectly cloudless, and the air calm; such were the conditions under which I witnessed a fine display of the Zodiacal Light. It was brighter and more conspicuous than I have ever seen it on any former occasion. Its colour was of a yellowish white. A line drawn from ρ to σ *Piscium* would show the extent of the brighter portion of the cone up to ν, μ, π *Arietis*. Another drawing of the appearance was made, as on the 9th of last month. I traced the boundaries on maps 2, 3, and 4 of Mr. Proctor's *New Star Atlas*, and was pleased to see that it could be done with sufficient accuracy to give a correct idea of the display. These two tracings I beg to enclose along with the above report, with the hope that they will prove acceptable, and will also excuse me for prolixity.

My place of observation was the same as that described in the *Monthly Notices*, Vol. xxxiii. p. 100.

Morges, Switzerland,
1874, *June.*

P.S.—Position of Morges approximately.

	°	′	″		
	46	30	0	N. Latitude.	
	6	31	0	E.	of Greenwich in Longitude (mean time).
	h	m	s		
or	0	26	4	fast	

Discovery of Minor Planet (138). By M. Perrotin.

This planet was discovered at the Observatory of Toulouse on May 19. The observed places on May 19 and 20 are as follow:

1874.	Mean Time at Toulouse. h m	R.A. h m s	N.P.D. ° ′
May 19	10 0	16 28 30	112 48
20	12 0	16 27 28	112 47

Definitive Elements of the Comets I. 1801; I. 1824; III. 1840; II. 1869. By Dr. Doberck, Astronomer at Colonel Cooper's Observatory, Markree Castle.

I. 1801. T = 1801, August 8·56305. M. Par. T.

$$\left.\begin{array}{lr} \pi & 182^\circ\ 41'\ 52'' \\ \Omega & 42\ 28\ 54 \\ i & 20\ 45\ 0 \end{array}\right\}\text{M. Eq. 1801·0.}$$

log q 9·40894

Retrograde.

I. 1824. T = 1824, July 11·514230. M. Par. T.

$$\left.\begin{array}{lr} \pi & 260^\circ\ 18'\ 4''\!\cdot\!1 \\ \Omega & 234\ 20\ 41\cdot0 \\ i & 54\ 36\ 44\cdot8 \end{array}\right\}\text{M. Eq. 1824·0.}$$

log q 9·771850

Retrograde.

III. 1840. T = 1840, April 2·421053. M. Par. T.

$$\left.\begin{array}{lr} \pi & 324^\circ\ 3'\ 5''\!\cdot\!2 \\ \Omega & 186\ 2\ 7\cdot3 \\ i & 79\ 52\ 29\cdot8 \end{array}\right\}\text{M. Eq. 1840·0.}$$

log q 9·8743460

Direct.

II. 1869. T = 1869, October 9·856133. M. Par. T.

$$\left.\begin{array}{lr} \pi & 139^\circ\ 42'\ 33''\!\cdot\!25 \\ \Omega & 311\ 30\ 10\cdot4 \\ i & 111\ 40\ 21\cdot5 \end{array}\right\}\text{M. Eq. 1869·0.}$$

log q 0·09016325

Périodes de Révolution de 4 étoiles doubles, d'après les dernières Observations. Par M. Flammarion.

	Ans.	Demi gr. axe app. ″	Passage Périhélie app.
ξ Ursæ Majoris	60·60	2·45	1873·40 à 358°
ζ Herculis	34·57	1·19	1864·35 à 298
η Coronæ Borealis	40·17	0·865	1853·95 à 287
γ Virginis	175·	3·389	1836·45 à 320

M. Flammarion remarks that, owing to the large number of observations used in the calculation, he believes the above periods to be absolutely exact.

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

Vol. XXXIV. Supplementary Notice. No. 9.

Account of a MS. Table of Twelve Figure Logarithms of Numbers from 1 to 120,000, *calculated by the late Mr. John Thomson, of Greenock, and recently presented to the Royal Astronomical Society by his sister, Miss Catherine Thomson.* Drawn up, at the request of the Council, by J. W. L. Glaisher, M.A.

§ 1. *Description of the MSS.; Mode of Calculation, &c.*

In the course of last June (1873) Mr. Andrew Williamson, of Greenock, wrote to the Secretaries of the Royal Astronomical Society, with reference to a large MS. Table of Logarithms, calculated by the late Mr. John Thomson, of that town, and which, under certain circumstances, his sister, Miss Catherine Thomson, was willing to present to the Society. Mr. Williamson's letter was accompanied by one from Mr. Thomas Cranston, teacher of mathematics and navigation at Greenock, giving a short account of the contents of the MSS., from which it appeared that they consisted of a twelve figure Table of Logarithms, of numbers from 1 to 120,000, apparently the result of an independent calculation, together with a considerable portion of the work. Having been requested by the Council to act on their behalf in the matter, I wrote to Mr. Williamson and expressed an opinion that there was but slight chance of anyone being likely to undertake the publication of the table, which would necessarily entail a heavy pecuniary outlay, with very small return, but that the MSS. might be of high interest for purposes of reference. Thereupon, Miss Thomson at once presented the whole of the MSS. to the Society, expressing, through Mr. Williamson, her belief that she was thus rendering her brother's work as useful and accessible as possible, and was adopting the course which, had he lived, would have been most in accordance with his wishes. For this generous gift the thanks of the Society were returned to Miss Thomson, and also to Mr. Williamson and Mr. Cranston for their good offices, at the ensuing meeting, in November 1873.

Before describing the MSS. at length, which on examination appear to be a very valuable addition to the Society's library, it is proper to state the following facts relative to Mr. Thomson's life with which, at my request, I have been kindly furnished:—Mr. John Thomson was born at Strachur, Argyleshire, in 1782. His father, Dougal Thomson, farmer, married Margaret McKialay, by whom he had eleven children, John being the second. He was educated at Strachur till he was twelve years of age, when he went to Greenock, and was placed under the care of the late Colin Lamont, teacher of mathematics and navigation, from whom he imbibed a taste for mathematical studies. For many years he occupied the position of clerk to a firm in Greenock, but subsequently he commenced the business of an accountant. He died on December 31, 1855, and was buried in Duncan Street burying-ground, Greenock, where his grave is marked by no stone or monument. He was never married, and for some time before his death suffered from failing health. These few sentences contain all the facts which Mr. Williamson and Mr. Cranston could collect relative to Mr. Thomson's life. His sister, the donor of the tables, being his junior by more than twenty years, knows nothing of his early studies, and very little of his pursuits in the prime of life. The work undertaken by Mr. Thomson, and the manner in which he has performed it, testify more clearly than could anything else to his steady and methodical habits.

The MSS. consist of four volumes of results, eight volumes containing portions of the calculation, and three small covers and a tray containing slips. A more exact enumeration, giving also the letters with which I have marked (in red ink) the different parts of the MSS., for purposes of reference, is as follows:—

A. (4 Vols.) All the results; viz., a complete table of logarithms of numbers from unity to 120,005 (besides the logarithms of 744 higher numbers at irregular intervals, the highest being log 123,187) to 12 places of decimals. Vol. 1 (marked A_1, size of page 9 in. by 3·5 in.) extends from 1 to 25,694; and the contents of the other three vols. (marked A_2, A_3, A_4, quarto size) are 22,695 to 60,179; 60,180 to 91,200; and 91,201 to 120,005, with the 744 additional logarithms just mentioned.

a. (2 Vols.) Logarithmic differences. Vol. 1 (marked a_1, small quarto) contains the last nine figures of the logarithms, with first and second differences, for numbers from 29,561 (erroneously written on the cover 29,651) to 57,401; and vol. 2 (marked a_2, quarto) contains the last nine or six figures of the logarithms, with first and second differences, for numbers from 57,401 to 120,001 (the last nine figures are given as far as 71,201).

b. (2 Vols.) Logarithms of multiples. Vol. 1 (marked b_1, octavo) is entitled "Preparative Tables of Logarithms,

1st Part"; and vol. 2 (marked b_2, octavo) "Preparative Tables of Logarithms, 2nd Part, continuations." They contain logarithms of multiples of 3, 5, 7 . . . and of prime numbers up to 55,691 (of the higher numbers the logarithms of the doubles only are given).

c. (3 Vols.) Logarithms of higher multiples required in the portion of the table from 40,000 to 120,000. Vol. 1 (marked c_1, small quarto) is entitled "Logarithms, multiples of 3, Nos. 40,000 to 120,000," and contains logarithms of multiples of 3, the lowest multiple being 13,334 × 3 = 40,002, and the highest 40,001 × 3 = 120,003; Vols. 2 and 3 (marked c_2 and c_3, small quarto) are respectively entitled "Logarithms, multiples of 5 to 101, Nos. 40,000 to 120,000," and "Logarithmic Multiples of 103 to 15,013, Nos. 40,000 to 120,000," and are similar in their contents to c_1, the primes whose logarithmic multiples are given being indicated in the titles.

d. Three small covers of stiff paper, and a tray, containing slips with the logarithms used in the formation of *b* and *c* written upon them. The three covers (marked d_1, d_2, d_3, size 8 in. by 2½ in.) are entitled respectively, "Logarithmic Slips, Nos. 1 to 2,000," "Logarithmic Slips, Nos. 2,000 to 4,000," "Logarithmic Slips, Prime Nos. above 4,000." The little cardboard tray (marked d_4, same size as the covers) contains the logarithms of the lower numbers, written each on a separate slip of cardboard.

e. (1 Vol.) A small MS. of 34 leaves, octavo, entitled, "Numbers distinguishing primes from composites," and giving the factors (2 and its powers being omitted) of the numbers up to 21,460.

There are besides several other loose MSS. that will be referred to further on (see the end of this section); one, consisting of 16 small leaves stitched together, gives several powers of a few primes and their logarithms.

The general method followed by Mr. Thomson in his calculations is evident from the above enumeration, viz., the logarithms of the primes having been obtained, the logarithms of the composite numbers were deduced by addition, and the whole was verified by differences. There is not, however, among the MSS. anything that relates to the calculation of the logarithms of the primes, nor is there to be found a single formula or piece of algebra having any reference whatever to logarithms. All the papers that the Society possesses merely concern the formation of the logarithms of the composite numbers by the addition of the logarithms of their factors, and there is no direct evidence of the method pursued with regard to the primes. For reasons that will be stated hereafter, however, there can be but little doubt that the logarithms of these were also calculated *de novo*, although the work has been lost. Leaving, then, for the present, the primes out of the question, and assuming the author to have been in

the possession of their logarithms, the rest of the procedure is clear. The first thing was to transcribe the logarithms of 2, 3, 5 . . . upon separate slips of paper, and so by addition obtain the logarithms of the composite numbers up to some limit, say 1,000; then, having obtained the logarithms of these first thousand numbers, by adding them to log 3, we obtain the logarithms of the first thousand multiples of 3, by adding them to log 5, of the first thousand multiples of 5, and so on. The slips so used are contained in *d*,* and the additions performed by means of them are given in the 5 vols. *b* and *c*. It would be impossible to explain the exact course pursued, which seems to have been guided by no fixed rule. Thus, in b_1 we have logarithms of multiples of 3 up to 385 × 3 (= 1,155), of 5 up to 207 × 5 (= 1,035), of 7 up to 358 × 7 (= 2,506), of 11 up to 156 × 11 (= 1,716), of 13 up to 151 × 13 (= 1,963)..., of (say) 11,939 up to 7 × 11,939 (= 83,573), &c., &c. Many multiples are, however, omitted—thus, in 3, 260 × 3; 262 × 3; 263 × 3; 267 × 3; 268 × 3 are consecutive multiples; in 7, 31 × 7; 32 × 7; 34 × 7; 37 × 7; 38 × 7; 40 × 7 are consecutive, and so on. No particular principle seems to have governed these omissions, the object of which, of course, was to prevent the same logarithm being calculated several times over, as a multiple of each of its factors; but no attempt was made to carry this out rigorously, as many logarithms do, in fact, appear twice over, and some even more frequently. In b_2 we have the continuation of the multiples in b_1; thus, the multiples of 3 begin at 386 × 3 (= 1,158), and end at 13,376 × 3 (= 40,128); the multiples of 5 begin at 209 × 5 (= 1,045), and end at 8,001 × 5 (= 40,005), and so on. The vols. c_1, c_2, c_3, contain the higher multiples required in the portion of the table from 40,000 to 120,000. The vols. b_1 and b_2 do not contain multiples that exceed 40,000 of

* A more exact enumeration of the contents of the covers, &c., marked *d*, is as follows: d_1 contains 45 slips, giving the logarithms of the numbers from 1 to 2,000 (15 places); nearly all have writing on both sides, and when this is the case each gives about 50 logarithms; a few of the slips near the beginning having become worn out by use, are re-written, and so appear twice. d_2 contains 40 similar slips, extending from 2,000 to 4,000. d_3 contains 51 slips, giving the logarithms of primes, the lowest being 4,001, and the highest 30,319 (12 places). Each slip has been folded into creases after every logarithm, as would be required in adding them consecutively to the constant logarithms at the tops of the pages in *b*; and even now the slips present a wavy appearance, which, not less than their worn aspect, shows how much they have been used. The little tray d_4 contains 60 small slips of cardboard, of which 25 have a number and its logarithm on one side, and the other blank; and 35 have a pair of numbers and their logarithms, one on each side (in these cases the second number which appears is the number whose logarithm is to be found on the back, thus re-written to avoid the necessity of turning over the slip to see what is given there). The numbers range from 2 to 341, running regularly (multiples of 10 omitted) to 65; thence by uneven numbers to 95, above which there are only fourteen numbers (12 or 15 places), and a few occur twice. These logarithms of low numbers were doubtless thus written separately on cards on account of their more frequent use.

numbers up to 11,489, but above this limit the multiples are given as far as 80,000, and for numbers above (about) 20,000 they are given to over 100,000 (in fact, the first 8, 7, 6, 5... multiples are given, with omissions, according to the magnitude of the numbers); and c_1, c_2, and c_3 are therefore complementary to them. The factor table e was no doubt employed to determine what multiples might be omitted, besides being otherwise useful. It is no fault whatever that the multiples chosen are not subject to exact rules; it would clearly be a great deal more difficult and inexpedient to devise and carry out a scheme by which no composite number should appear twice, than to proceed as Mr. Thomson has done, leaving out only what he saw would be calculated in another part of the work, though of course making these omissions capriciously.

The logarithms being thus obtained, they were entered in order in the books A, and the last nine and six figures of them were also entered in a_1 and a_2, and the first and second differences taken, the errors so found being also corrected in A. An examination of the errors thus corrected renders it probable that the entries were made in A and a at the same time, but there is no reason why in certain parts of the table the logarithms should not have been copied, a good many at a time, from A into a. That such was not invariably the case is shown by the fact that certain logarithms (generally those of primes) have clearly sometimes been entered, both in A and a, at a different time from the majority of logarithms in their neighbourhood. At all events, all the logarithms above that of 29,561 have been second-differenced, and the errors so found carefully corrected in A; also, as it is not likely that Mr. Thomson should have commenced to difference at the number 29,561, it is fair to conclude that the earlier logarithms (from perhaps that of 10,000) were also second-differenced in a volume of the MSS. that has been lost.

It now remains to consider whence Mr. Thomson derived the logarithms of the primes. In the first place, there can be little doubt that the logarithms of the primes under 4,000 (which in b_1 are given to 15 places) were the result of an original calculation, as, I believe, there is no printed table that contains such high primes to so many as 15 decimal places; and the internal evidence also strongly supports this opinion. In 1717, Abraham Sharp published his "Geometry Improved," in which he gave the logarithms of all primes less than 1,100 to 61 places, and this table has several times been reprinted in other works, generally to a less number of figures, and with the addition of the logarithms of the intervening composites. Thus Callet gives the logarithms of the first 1,200 numbers to 20 places, and I accordingly read Mr. Thomson's logarithms of primes up to this extent with a Callet of 1795, the 20 places being supposed contracted to 15. There are 196 primes below 1,200, and (assuming Callet in all cases to be accurate) the discrepancies were as follow:—

The MS. was correct		in	8	instances
„ „ incorrect by	1	„	21	„
„ „ „	2	„	20	„
„ „ „	3	„	31	„
„ „ „	4	„	42	„
„ „ „	5	„	35	„
„ „ „	6	„	22	„
„ „ „	7	„	14	„
„ „ „	8	„	2	„
„ „ „	9	„	1	„

The errors are of course expressed in units of the last (fifteenth) place. To determine whether the errors increased in amount as the numbers became larger, the 196 primes were divided into two groups, the one containing the first 98 (viz., from 2 to 521 inclusive) and the other, the second 98 (viz., from 523 to 1193), with the following results:—

In the first group of 98					In the second group of 98				
The MS. was correct		in	7	instances	The MS. was correct		in	1	instance
„ „ incorrect by	1	„	17	„	„ „ incorrect by	1	„	4	instances
„ „ „	2	„	13	„	„ „ „	2	„	7	„
„ „ „	3	„	15	„	„ „ „	3	„	16	„
„ „ „	4	„	15	„	„ „ „	4	„	27	„
„ „ „	5	„	18	„	„ „ „	5	„	17	„
„ „ „	6	„	7	„	„ „ „	6	„	15	„
„ „ „	7	„	6	„	„ „ „	7	„	8	„
					„ „ „	8	„	2	„
					„ „ „	9	„	1	„

From these lists the increase in inaccuracy may be fairly estimated. Mr. Thomson's values were invariably too small, with only three exceptions, viz., log 79 and log 487 were too large by unity, and log 223 was too large by 5.

This comparison points very clearly to Mr. Thomson's values being the result of an original calculation, and it also shows how little he has availed himself of existing works as a means of verification. Briggs (1624) gives the logarithms of the first 20,000 numbers to 14 places, and the logarithms of the first 1,200 numbers have been published to a great many places in several works, but no fifteen figure table has appeared of numbers between 1,200 and 4,000. Also, it is unlikely that results so inaccurate in the last figure should have been derived from a printed source. As the basis of a twelve figure table, they are just such as we should expect them to be, and are abundantly accurate for the

purpose, but it is not probable that anyone would print them without reducing the number of places. Had Mr. Thomson known of Sharp's table (which he would have had no difficulty in consulting, as it is reprinted in Sherwin, Hutton, and Callet), it seems almost inconceivable that he should not have corrected his own by means of it: no one can imagine a computer wilfully using erroneous values merely because they are the results of his own calculation, when he has the correct ones before him—so that there is strong evidence that the logarithms of the primes under 4,000 were calculated *de novo*, without help from, or reference to, any published table. This is confirmed by the rest of the MSS., in which I can find no trace whatever of any assistance having been derived by Mr. Thomson from any published work or source independent of his own calculation. It seems strange that anyone should have undertaken to do with his own hand so large a piece of work without having previously studied the existing tables; but I am inclined to believe, that, having read with interest the properties of logarithms, and met with an account of the mode of construction of a table; and having doubtless seen seven figure logarithmic tables (and perhaps heard of Briggs's imperfect fourteen figure table and Vlacq's ten figure table), he determined to devote his time to the independent calculation of a complete twelve figure table, mainly on the ground that it would be larger than any that had been calculated before.

During all Mr. Thomson's working lifetime Hutton's logarithms were the standard table, and it seems improbable that he should not have possessed a copy of so well-known a work; which, moreover, was well calculated to awaken interest in the subject on account of its full historical introduction. But all the early editions contained Sharp's table of logarithms of primes under 1,100, which Mr. Thomson does not appear to have seen, and of which he certainly made no use. At all events, whether as the result of want of knowledge, or of a strong desire to do everything with his own hand without being under obligation to any other computer, it seems pretty clear that he undertook and accomplished successfully by himself the whole labour of constructing a much larger table of logarithms of numbers than any that has been published, entirely from first principles, as if no logarithms had been calculated previously. There are, no doubt, cases on record, of persons who, from mere love of computing (*i.e.* delight in performing the calculations, rather than desire for the attainment of the results on account of their value) have performed similar pieces of work, and there are at present not a few unfinished MS. tables of logarithms in private hands that have been so undertaken; but there seems some reason to attribute to Mr. Thomson a higher object, and to suppose that having determined to construct the table, he carefully considered what its extent should be, so as to render it as valuable as possible relatively to the work bestowed on it. The extension of the table to 120,000 instead of 100,000 may, of course, have been

prompted merely by a desire to excel what had been done, but it may also have resulted from a knowledge of the inconveniences that attend the use of the existing tables, when the first figures begin with 10... or 11 ... and second-differences have to be taken into account.

It being thus nearly certain that the primes under 4,000 were computed afresh, it is highly probable that the others were so too, but the manner in which this was done is not very evident. Supposing the logarithms of all the composite numbers to have been found, written out, and (wherever possible) differenced a first and second time, it is clear that it would generally have been easy to have then obtained the logarithms of the primes by mere addition from the differences. I have therefore carefully examined the two books a_1 and a_2 that contain the differences, to see what internal evidence there is in favour of or against this course having been followed, but I have not been able to find anything that throws any light on the matter. In not a few cases there were corrections both in the numbers and differences, but these were always capable of explanation in both ways, viz. either on the supposition that the primes were copied from some other MS. book and differenced for a verification, or that they were obtained originally by means of the differences. In many places the logarithms of the primes and the corresponding differences have evidently been inserted at a later period than those of the composites in the vicinity; but this affords no clue to the mode of calculation of the former, as, however they were obtained, it is certain that the work was kept quite distinct from that relating to the composites. I rather incline to the opinion that the logarithms of the primes were not calculated from the second differences, but were obtained by some more independent method and differenced merely as a verification, the same as was the case with the composites. In both a_1 and a_2 the primes are marked with a pencil *P*,* and near the end of a_2 many of the composite numbers have one of their factors written before them. In the other MSS. *P* appended to a number *generally* denotes that it is a prime.

* The numbers so marked are not, however, all primes. I read the two chiliads 79,000 – 81,000 with the primes in Chernac, and found the following composites marked with a pencil *P*. :—

79,369 (= 139·571)	79,789 (= 73·1093)	80,773 (= 7·11·1049)
79,463 (= 229·347)	79,921 (= 229·349)	80,837 (= 229·353)
79,667 (= 7·19·599)	80,131 (= 227·353)	

while 80,701, which is a prime, was not marked. It seems unquestionable that the pencil *P* was intended to denote a prime, and we must either suppose that for some reason the above composites were treated as primes in the calculation, or that Mr. Thomson believed them to be primes, or, what comes to the same thing, found it less trouble to treat them as primes than to determine whether they were so or not. In any case, the inference is, that Mr. Thomson had not in his possession an extended factor table or list of primes.

The factor table *e*, extending to 21,460,† may have been the result of original work, as although Maseres (1795), Vega (1797), Lambert (1798), and others, have given such tables up to 100,000 or 102,000, while Chernac's table (1811) extends to over a million, and Burckhardt's (1817) to three millions, Mr. Thomson may not have been acquainted with these works. With regard to the primes, besides the full lists contained in nearly all the works just mentioned, Barlow, in his *Mathematical Tables* (1814), gave a list of primes up to over 100,000, and in *Rees's Cyclopædia* (1819) there is a list up to 217,219, so that tables of primes were readily accessible in this country at the time Mr. Thomson was engaged upon his calculations.

The book A_4, containing the final portion of the completed results, is ruled for the logarithms throughout, and the numbers are written in as far as 123,187, which occupies the last line in the book; but the logarithms are only entered regularly (*i.e.* without omissions) as far as 120,005, above which there are very numerous blanks. It is evident, however, that Mr. Thomson had all along fixed 120,000 as the limit of the table, as the work is throughout performed on the supposition that higher multiples would not be required, and it is probable that he contemplated the addition of the next 3,000 logarithms, merely because there happened to be room for them in the book A_4. Beyond 120,000 we have, as it were, the table in process of formation, and no doubt a very attentive study might help to elucidate more fully the mode of procedure followed; but an ordinary examination shows nothing that could not be pretty nearly as well seen from the earlier portions of the table.

No one who examines the MSS. can fail to be struck by the great uniformity and neatness of the work. Though the calculation must have extended over several, and perhaps many years, and much of it must have been performed when Mr. Thomson was fatigued, there are no traces of any change of style or signs of haste; the figures are invariably made with the same clearness, and of the same uniform size. The work seems to have been done throughout in a slow, methodical way, and very accurately (viz. the alterations are very few). It is also everywhere condensed; for had the logarithms been copied out and so added, instead of the slips in *d* being used, the extent of the MSS. would have been tripled. This ought to be noticed particularly, as anyone merely looking at the MSS. might easily much underrate the amount of labour involved. To show the closeness of the writing, it is worthy of remark, that the four volumes *A* contain more figures than does Vlacq's large folio of 1628. One singular peculiarity in the work is, that Mr.

† It is a coincidence that Köhler's *Logarithmisch-trigonometrisches Handbuch* (Leipzig) contains a factor table up to 21,524, differing very little from Mr. Thomson's limit, viz., 21,460. The copy of Köhler that I have seen is of the second stereotyped edition (1848), and I do not know when the book was first published.

Thomson never writes a logarithm without its index; the columns are always headed "Number—Index—Logarithm." It seems strange that, for the sake of a fancy, he should have been willing to make so many thousands of useless figures, but this scrupulousness in a mere matter of form is, to a certain extent, characteristic of the work. Mr. Thomson had, no doubt, the logarithms continually present to his mind as exponents, and he was reluctant to commit to writing anything that from his point of view did not seem to be absolute truth. The attention so laboriously and perseveringly paid to a crotchet would not be inconsistent with the character of one who had resolutely determined to perform the whole of the work he had proposed to himself without assistance of any kind from others.

Mr. Cranston mentions that some of the MSS. are known to have been destroyed, and it seems curious that all the work that has escaped should have reference solely to the composites; but this may be explained by the consideration that the calculations relating to the primes, being less capable of methodical arrangement, may not have been so carefully written in books.

The non-logarithmic MSS. of Mr. Thomson's, to which allusion has been made (and which, including the few leaves containing the logarithms of powers of primes, I have numbered in red ink from 1 to 16), do not possess any value. Most of them relate to chemistry, and consist of extracts from printed books (chiefly Ure's *Dictionary of Chemistry*), on the composition of vegetables, a chemical notation, specific gravities of gases, velocities of winds, weights and measures of different countries, &c. &c., besides some formulæ, and a small table relating to the fall of bodies *in vacuo*. The longest is entitled "Murray's Experiments Illustrative of Chemical Science." The whole weight of these non-logarithmic MSS. is just two ounces, and they are only of interest in so far as they throw light on Mr. Thomson's early tastes. Most of them were probably copied at a time much anterior to that at which he commenced the calculation of his logarithms.

§ 2. *Accuracy of the MSS.; Errors found in Briggs's hundred and first Chiliad.*

Assuming, therefore, as can scarcely admit of doubt, that Mr. Thomson's twelve figure table to 120,000 is the result of a wholly original calculation, the next points to be considered are:—(1) how far it may be relied on for accuracy; and (2) to what uses it may be put.

To determine the accuracy of the MSS. I have read parts of them with the existing tables that give more than 12 figures; but before stating the results of these comparisons, it is desirable to explain briefly the contents of the books, &c., which give the most extensive tables of logarithms of numbers that have appeared, and the corrections thereto. These works, which therefore come in close relation with Mr. Thomson's MSS., are—

(1) Briggs's *Arithmetica Logarithmica*, folio, London, 1624.

Fourteen figure logarithms of numbers from 1 to 20,000, and from 90,000 to 100,000. A very few rare copies give an additional chiliad, and extend to 101,000.

(2) Vlacq's *Arithmetica Logarithmica*, folio, Gouda, 1628 (and London, 1631, with an English title). Ten figure logarithms of numbers from 1 to 100,000.

(3) Vega's *Thesaurus Logarithmorum Completus*, folio, Leipzig, 1794. Reprint of Vlacq, with 1,000 new logarithms added, viz. ten figure logarithms to 101,000.

(4) Borda and Delambre's *Tables Trigonométriques Décimales*, 4to, Paris, 1800. Contains eleven figure logarithms of numbers from 100,000 to 102,000.

(5) The *Tables du Cadastre*, calculated by the French Government at the end of the last century, contain fourteen figure logarithms from 1 to 200,000, the result of an original calculation. There are two copies of the MS., one at the Paris Observatory, the other at the Institute.

(6) The *Annales de l'Observatoire de Paris*, t. iv. (1858), contains the results of a reading, by M. Lefort, of the *Tables du Cadastre* in the Paris Observatory library, with Briggs, 1624, and Vlacq, 1628. List of errata so found in both these works are given.

One or two other MSS., of which no use has been made in this report, will be referred to further on.

It will be seen that 30,000 logarithms given by Briggs in 1624 (or 31,000 in the special copies), to fourteen places, were recalculated by Mr. Thomson to two places less. I therefore selected three groups of a thousand each (viz. 0–1,000, 19,000–20,000, and 100,000–101,000), and had them read from the MS., while I followed in Briggs.

Results of the Comparison of the first thousand Logarithms in Briggs and Thomson.

As Briggs extends to fourteen places, while Thomson has only twelve, the former gives always the next two figures, and affords the means of testing the accuracy of the last figure in the MS. Leaving out for the moment the cases in which the last two figures in Briggs were 50, there were found to be thirteen cases in which (on the assumption that Briggs was correct) the last figure ought to have been increased by a unit. Of these thirteen cases the next figures were 51 in four, and 52, 53, 54, 57,* 59, 60, 61,* 63,* 66 in the other nine, cases. The logarithms of the first 1,200 numbers are given in Callet, and therefrom it appears that in one of these cases, viz. in log 933, where Briggs gives the last two figures as 51, the figures after the twelfth really are 49994... ; so that Thomson is wrong twelve times, the remaining discrepancy being due to Briggs. The number of cases in which the last two

* On reference to Callet, it is seen that 57, 61, and 63 should be 58, 60, and 62.

figures in Briggs were 50 amounted to ten, in none of which was Thomson's last figure increased; but on referring to Callet for figures beyond the fourteenth it appeared that Thomson was only wrong three times. Thomson has five times written $\frac{1}{2}$ at the end of the logarithms, and in these cases the next figures were really 62, 61, 66, 50, 59. It is thus seen that the MS. up to 1,000 is very accurate, even in the last figure, and that the largest *avoidable* error amounts to .32 of a unit in this last place. It will be noticed that, as was found with the primes, the MS. is always in error by defect. The logarithms from 0 to 1,000, forming the beginning of A_1, are not written nearly so neatly as those in the rest of the MS., corrections being numerous and not always clear. Thus the last two figures of log 651, which are 68, cannot be distinguished from 08.

The comparison detected three errors in Briggs, of which the first (in log 80) is corrected in Lefort's list in the *Ann. de l'Obs. de Paris*, and the last (in log 540) by Briggs himself in his own errata list; but the remaining one, viz. in log 234 (at the top of the column), where the first figure of the logarithm should be a 3, instead of a 2, seems, very curiously, not to have been noticed. Briggs gives a hundred logarithms on each page in three columns, and the logarithms which appear at the foot of the first two columns (and which correspond to numbers ending in 34 and 67), are repeated at the head of the next columns. But in making a comparison between Briggs and another work, these logarithms that occur twice would not be read twice, so that in a case where one contained a misprint, which did not appear in the other, the error might easily escape detection, and this is what has no doubt happened with this misprint in log 234.

Results of the Comparison of the Logarithms of Numbers from 19,000 *to* 20,000 *in Briggs and Thomson.*

Briggs gives no logarithms of numbers between 20,000 and 90,000, so that 19,000–20,000 is the nearest chiliad to the middle of the table that could be selected. The comparison of this portion showed that (supposing Briggs to be always correct in his last two figures, and omitting the cases where they were 50), the number of last figure errors in Thomson was 47, which were thus distributed:—

Amount.	Number of errors, positive.	Number of errors, negative.	Total.
·50– ·60	7	12	19
·60– ·70	1	9	10
·70– ·80	5	1	6
·80– ·90	4	2	6
·90–1·00	0	1	1
1·00–1·40	2	3	5
	19	28	47

The ranges in the amount column are to be taken inclusive of the first limit, and exclusive of the second, thus: ·50—·60 includes any error whose first figure is a 5. An error is considered positive when the erroneous value exceeds the true value, viz.:

$$\text{error} = \text{erroneous value} - \text{true value}.$$

It is to be remarked that the negative are more numerous than the positive errors; that is to say, that the values in the MS. are generally too small. The largest errors (1·39 and 1·40) occur in logs 19,727 and 19,801. In nine cases in Briggs the last two figures are 50, and in but one of these the twelfth figure is increased in Thomson. It is not worth determining whether the increase should have been made or not, as for all practical purposes both are equally correct (see *Monthly Notices*, vol. xxxii. p. 261, and vol. xxxiii. p. 450, May 1872, and May 1873). A computer who wishes to be quite accurate, even with a perfect table, must so arrange his work that x, y, z ... being the logarithms, and the decimal point referring to fractions of a unit in the last place,

$$f(x, y, z \ldots) = f(x + .5, y + .5, z + .5 \ldots)$$

to the number of figures he intends to retain, and he cannot make so delicate a distinction as is involved in discriminating between this and

$$f(x, y, z \ldots) = f(x + .55, y + .55, z + .55 \ldots)$$

Thus, both from the points of view of the calculator and the user of the table, discrepancies where the next figures are 50, 50 ± 1, or even 50 ± 5, cannot practically be regarded as errors. But as, throughout a good deal of his work, Mr. Thomson has only gone to twelve places, he does not in these cases deserve the credit for accuracy that belongs to those who reject additional figures, and therefore *only* make "errors" of this kind. In one instance (log 19,052) there is a 5 inserted at the end; the next two figures are really 56.

In this chiliad occurs the only real blunder (*i.e.* error, not in the last figure, and produced by the mode of calculation) that I have met with in the three chiliads (but two more in another part of the table will be pointed out in the next section). The logarithm of 19,620 is ·292 699 003 044, and not ·292 690 003 044, as given by Thomson. The book a_1 commences at 29,651, so that it cannot be decided whether this error was passed in the differences. The logarithm of 1962 is correctly given: it is possible that the error may have arisen in transcribing log 1962, where the tail of the 9 is a little separated from the body. The comparison brought to light ten errors in Briggs, but they were all found to be corrected in his errata list. I may mention that Briggs gives an erratum in a logarithm (log 19,677) in this chiliad that can only apply to

a part of the impression, as in two copies before me this logarithm is accurately printed, and needs no correction.

Results of the Comparison of the Logarithms of Numbers from 100,000 *to* 101,000 *in Briggs and Thomson.*

As mentioned above, in a few copies of Briggs there is an additional chiliad at the end containing the logarithms of the numbers from 100,000 to 101,000, and it seemed preferable to read this last chiliad with the MS. rather than any other, not only on account of its being the concluding portion of the fourteen figure table, which has never been carried beyond 101,000, or indeed extended since Briggs left it, but also because, even supposing that Mr. Thomson ever did possess or see a Briggs, it is very unlikely that he should have lighted upon one of the rare copies that contained this supplement. Supposing, as before, the last two figures of Briggs to be always correct, the number of last-figure errors (and there were no others) found in Thomson was 115, which were thus distributed :—

Amount.	Number of errors, positive.	Number of errors, negative.	Total.
·50- ·60	18	21	39
·60- ·70	19	13	32
·70- ·80	8	6	14
·80- ·90	9	6	15
·90-1·00	0	3	3
1·00-1·23	8	4	12
	62	53	115

So that in this chiliad the values in the MS. were generally too large. The largest error (1·23) occurs in log 100,461. In eight instances the last two figures in Briggs were 50, and in four of these the twelfth figure was increased in Thomson.

Briggs in his own errata-list only gave errors found in the portion of the table below 20,000; and Lefort's list, formed by comparison with the *Tables du Cadastre*, contains none above 100,000; in fact, there can be no doubt that neither Lefort nor Prony (who first had the comparison made) possessed a copy of Briggs containing the extension beyond 100,000. The following errata that were detected in the hundred and first chiliad of Briggs have therefore probably not been published previously :—

Number.	Error.	Correction.
100,008	37421	47421
100,253	77377	73774
100,359	63262	63252
100,499	17203	17403
100,555	00239	00240
100,584	89027	89025
100,834	66956	69956

The last error is repeated, as 100,834 occurs both at the bottom of the first column and at the top of the second; but the latter (viz. the logarithm at the top of the second column) contains a fresh error, the last four figures being printed 2792 instead of 2782, as it is clear they should be from the differences. In all the seven cases noted above the differences were found to be correct. There is a very important printer's error affecting the whole of the last column but one in the chiliad, except the first and last logarithms, viz., extending from log 100,934 to log 100,966. This is caused by 100,934 being printed twice over in the number column, and 100,966 being omitted; thus, except the first and last, all the numbers in the column should be raised a line. By the comparison with Mr. Thomson's MS. the differences were of course not verified, neither were the thirteenth and fourteenth figures of the logarithms; the following errors in the differences were, however, met with while making the comparisons referred to hereafter:—

Number.	Error.	Correction.
100,119	6110	6119
100,482	43320	43220

Also, besides the erroneous column of numbers already noticed, errors were remarked in the following numbers:—

100,155	100,690	100,855	100,990
100,190	100,755	100,890	
100,655	100,790	100,954	

On the whole, then, the examination of the three chiliads disclosed only one blunder in the MS., and no last-figure error exceeding a unit by contraction (the largest being 1·40). Leaving out the cases where the next two figures in Briggs were 50, at the commencement of the table 98·8 per cent. of the logarithms were found to be correct; at 19,500, 95·3 per cent.; and at 100,500, 88·5 per cent.; so that the last figure throughout is generally to be relied upon, and appears never to be wrong by more than a unit. It is noticeable, also, that although the number of errors becomes greater as the numbers increase, their magnitude shows no tendency to do so, at all events above 20,000.

It is almost needless to remark that, leaving last-figure errors out of consideration, it would not be fair to say that Thomson was more accurate than Briggs, as most of the errors in the latter are due to the printer.

§ 3. *Errors found in Briggs, Borda, Vlacq, and Vega, by comparison with Mr. Thomson's MS.*

The characteristic features of the MS. are, that the logarithms are given to twelve places, and that they extend to 120,000; and not until a new ten figure table—with range 12,000 to

120,000—is in course of publication, can all the advantages which the MS. affords be brought into use. To the editor of such a table Mr. Thomson's work would be of much service, and enable him to guarantee the accuracy of the last figures, and also of the 20,000 additional logarithms. At present, however, the only purpose to which it can be put is the detection of errors in the existing tables, and I have accordingly so applied it in the manner which will be now described.

Considering first only the logarithms of numbers that exceed 100,000, the two tables in need of verification are Borda and Delambre, and Vega. As soon as the decimal division was declared by the French Government to be one of the fundamental articles of the new system of weights and measures, Borda applied himself to the calculation of new decimal tables, and in 1792 had completed his MS. Several causes delayed its publication, and at the time of his death a portion was still in the press. The printing was completed, and an introduction written, by Delambre, who edited the work in 1800 (an. ix.) under the title *Tables Trigonométriques Décimales*. Among the tables is one giving to eleven places the logarithms of the numbers from 100,000 to 102,000. There is no information to be found regarding it, but there can be no doubt that it was calculated by Borda himself. The first thousand logarithms (100,000-101,000) were given in the special copies of Briggs to fourteen places, so that over this portion the comparison was made between Briggs and Borda, and the corresponding values in Thomson were then referred to for confirmation of Briggs. (The comparison of this chiliad between Briggs and Thomson had been already made; see § 2.) The following were the discrepancies:—

No.	Borda.	Briggs.	Thomson.
100,119	8	7500	75
175	1	0474	05
231	1	1515	15
296	5	5524	55
331	8	8508	85
376	9	8482	84
378	6	5164	52
399	4	4522	45
489	3	3551	36
490	2	2517	25
534	0	0539	05
543	5	5534	55
592	0	0610	06
618	4	3499	35
649	0	0525	05
676	9	8453	85

No.	Borda.	Briggs.	Thomson.
100,679	5	4264	43
688	1	0339	03
697	10	09428	094
727	4	4500	45
747	7	7500	75
880	6	6504	65
939	7	7508	75
942	1	1507	15

There is also an important misprint in log 100,803, which appears in Borda as 00347245 . . . instead of 00347345 . . . When the next figures are 500, Borda is quite as likely to be right as not, and in any case he is guilty of no error. It will be observed that as far as possible Briggs is confirmed by Thomson, so that in most cases there can be no doubt of the true result. I calculated log 100,618 to fourteen places, and (assuming Briggs's value of log 7187 to be correct) found the last four figures to be 3498. The comparison of the chiliad from 101,000 to 102,000 showed the following discrepancies:—

Number.	Borda.	Thomson.	
101,079	8	86	(8446)
179	5	56	(5561)
194	5	56	(5575)
241	1	17	(1692)
767	10	094	(09381)

I calculated the logarithms of these five numbers to fourteen places, and have placed the last four figures in parentheses in the right hand column; they confirm Thomson's values in every instance except the first. In 115 cases in this chiliad the final figure in Thomson was 5, and in sixty-two of these the final figure was increased by contraction, and in fifty-three not increased, so that the accuracy of Borda's calculation is here also manifest.

The comparison of this chiliad detected two large errors in Thomson, viz. log 101,051 is given as ·00444 . . . instead of ·00454 . . . and log 101,068 as ·00451 . . . instead of ·00461 . . . These errors—no doubt due to a false addition—are not shown in a_2, as only the last six figures are there differenced.

In his *Thesaurus Logarithmorum Completus* (fol. 1794), Vega reprinted Vlacq's ten-figure table, and added a fresh chiliad, viz. from 100,000 to 101,000, which was calculated for him by Lieut. Dorfmund. This chiliad I read with Briggs, and then referred in all cases of discrepancy to Thomson for confirmation. The result showed that in no less than 280 instances was the last figure erroneous by a unit, and that in three instances it was in

error by two units. The logarithms of the 147 numbers contained in the following lists are too small by a unit in Vega, viz. the values given by Vega should be increased by a unit:—

100,021	100,148	100,363	100,539	100,737	100,862
022	149	365	563	739	863
024	153	368	564	743	865
025	165	369	566	744	867
035	167	371	568	774	868
036	211	373	569	778	883
038	213	382	585	779	884
045	214	383	589	801	886
047	236	405	591	802	888
058	237	406	592	804	889
059	262	408	593	805	895
061	264	457	594	806	896
074	266	461	595	807	914
075	267	462	596	808	916
077	268	463	597	809	917
078	269	464	599	811	918
088	271	465	603	812	919
089	272	466	654	841	921
091	287	467	655	842	922
104	288	491	657	848	944
105	296	492	733	849	945
107	297	496	734	851	947
118	301	535	735	853	956
119	361	536	736	854	957
121	362	538			

The logarithms of the 133 numbers contained in the following list are too large by a unit in Vega, viz. the values given by Vega should be diminished by a unit:—

100,003	100,141	100,279	100,485	100,679	100,788
004	142	307	486	691	817
007	143	328	487	692	824
009	144	330	494	693	834
041	157	333	502	694	835
051	159	334	542	695	836
052	183	335	544	712	837
065	184	336	545	714	874
066	185	337	546	726	875

100,095	100,186	100,338	100,554	100,727	100,904
096	187	339	555	728	905
097	188	356	556	754	906
098	222	357	557	756	907
111	223	358	558	764	908
112	224	377	559	765	932
124	225	412	575	766	933
129	226	413	576	767	934
132	227	414	577	768	979
133	228	415	578	769	981
134	252	472	635	785	993
136	253	474	637	786	994
137	277	484	677	787	995
138					

The logarithms of the three numbers,

100,364 100,366 100,367

as they appear in Vega, should be increased by two units in the last place.

In every one of the 283 discrepancies Briggs is confirmed by Mr. Thomson's MS., so that no doubt can exist about the above errata, but in seven instances the confirmation is doubtful, viz., for the numbers 100,007; 100,157; 100,357; 100,367; 100,545; 100,694; 100,824, in which the last four figures in Briggs are 4976; 4992; 4965; 5034; 4940; 4949; 4961 respectively; while in Thomson the last two are in each case 50. Most of the errata are such as Vega should have avoided, as in only eighty cases is the eleventh figure 4 or 5. It is curious that an editor who devoted so much labour to secure accuracy in the portion of the table that he reprinted from Vlacq should not have taken steps to ensure more correctness in the part for which he was himself responsible; or, at all events, should not have stated how far his last figure was trustworthy. There is one misprint in the chiliad, viz., the last two figures of log 100,456 should be 12 instead of 72.

But, after all, the tables of logarithms of numbers above 100,000 are, on account of their limited extent, and for other reasons, but little used; and the chief value of the MS. lies in its affording a verification of the ordinary ten-figure table to 100,000, and especially of the last figures. In 1858 M. Lefort read the whole of Vlacq with the *Tables du Cadastre*, and published in the *Ann. de l'Obs. de Paris* (t. iv.) a list of all the errors so detected in the former. This list is of the highest value to the user of ten-figure logarithms, and by its preparation and publication M. Lefort has anticipated a very great benefit, which otherwise the present MS. might have been made to confer upon

calculators. Had not M. Lefort made the comparison with the *Tables du Cadastre*, there can be no question that it would have been most desirable, and well worth the labour, to have read Mr. Thomson's MS. with Vlacq; but as it is, considering that such an examination could merely bring to light the few stray errors that may have escaped M. Lefort, it is very doubtful whether it is worth while to read nearly a million and a half of figures merely for this purpose. It is also to be noted that, although in making the comparison any clerk will do to read the numbers, it is essential that he who "hearkens" should be perfectly trustworthy, and have more than a perfunctory interest in the attainment of accuracy. Thus, although anyone editing a reprint of Vlacq would feel no doubt at all about the expediency of reading his proofs with Mr. Thomson's MS., it does not appear necessary to undertake a piece of work that would occupy so much time, at present, when there is no immediate prospect of a reprint.

But there is one important service that the MS. can render without any great expenditure of time being required, viz. the verification of the accuracy of M. Lefort's list. I accordingly had the whole of the logarithms indicated as erroneous by M. Lefort examined in Vlacq and the MS., and found that in every case (with one or two insignificant exceptions, to be mentioned further on) the latter confirmed the *Tables du Cadastre*. The errors in Vlacq are of two kinds, viz. large errors, chiefly due to misprints, and at once seen to be such when pointed out; and last-figure errors, due to the calculation not having been carried far enough to invariably secure complete accuracy to ten places. I have always very much regretted that M. Lefort in his list—no doubt to save space—did no more than merely indicate the error and correction in the latter case, so that the reader had no opportunity of inferring anything with regard to the amounts of the errors, or deciding between those that were important and those that were not. In the *Notices* for May 10, 1872, I remarked that "probably a good many of Lefort's errata were of this class," viz. due to the contraction in cases where the next figure was 5, and therefore of very little practical consequence; and anyone who uses ten-figure logarithms, and corrects his table by M. Lefort's list, must feel a desire for information as to the magnitude of the errors. I therefore carefully went through the whole list, selected every correction that had reference to the last figure, and placed side by side the last figure in Vlacq and the last three in the MS., inserting an additional column containing the errors and corrections as given by Lefort. In this way the following table was formed:—

Number.	Vlacq.	Thomson.	Lefort. Err.	Lefort. Corr.	Number.	Vlacq.	Thomson.	Lefort. Err.	Lefort. Corr.
10033	3	246	3	2	27586	8	872	8	9
11003	9	960	29	30	27861	2	268	2	3
11240	3	233	3	2	27921	7	633	7	6
15620	6	541	6	5	28486	9	952	699	700
17646	8	891	8	9	28680	9	996	69	70
17647	6	691	6	7	29112	5	578	5	6
17648	0	096	0	1	29163	8	850	8	9
17649	0	107	0	1	29226	9	956	799	800
20071	0	936	10	09	29446	7	751	7	8
20280	6	661	6	7	29639	8	750	8	7
20375	5	412	5	4	29703	3	231	3	2
20645	3	211	3	2	30499	0	950	6000	5999
20822	2	141	2	1	30502	8	740	8	7
20866	1	046	1	0	30728	1	157	1	2
21245	5	426	5	4	31001	2	108	2	1
21749	2	270	2	3	31627	5	579	5	6
21795	5	353	5	4	31653	6	756	6	8
21904	9	790	9	8	31735	6	650	6	7
22016	7	555	7	6	31817	9	955	79	80
22200	4	451	4	5	31919	8	749	8	7
22312	2	258	2	3	32111	5	566	5	6
22877	1	180	1	2	32633	9	958	09	10
22996	9	974	2999	3000	32672	5	407	5	4
23274	9	984	299	300	33370	6	655	6	7
23492	3	171	3	2	34037	6	654	6	7
23820	2	147	2	1	34162	4	350	4	3
24156	0	937	10	09	34358	4	350	4	3
25173	9	811	9	8	34664	1	026	1	0
25524	9	950	59	60	34702	4	451	4	5
25586	5	551	5	6	34734	9	950	7999	8000
25707	5	550	5	6	35053	8	850	8	9
26004	3	367	3	4	35298	7	771	7	8
26188	2	250	2	3	38051	9	741	9	7
26407	5	446	5	4	38277	1	153	1	2
26642	9	965	39	40	38321	7	623	7	6
26717	4	458	4	5	38578	6	754	6	8
27291	5	430	5	4	38783	3	231	3	2
27560	3	236	3	2	39227	4	454	4	5

Number.	Vlacq.	Thomson.	Lefort. Err.	Lefort. Corr.	Number.	Vlacq.	Thomson.	Lefort. Err.	Lefort. Corr.
39626	.	200	.	2	50601	7	650	7	6
39802	5	369	5	4	50828	3	250	3	2
39839	7	649	7	6	50937	1	050	1	0
40108	2	269	2	3	50996	5	450	5	4
40127	9	950	19	20	51037	3	250	3	2
40909	6	517	6	5	51096	2	150	2	1
40966	6	665	6	7	51175	4	335	4	3
41121	4	495	4	5	51388	5	450	5	4
41156	5	550	5	6	51389	7	650	7	6
41227	2	277	2	3	51606	1	050	1	0
41385	6	535	6	5	51607	6	550	6	5
42584	1	165	1	2	51820	7	650	7	6
44021	0	949	40	39	51915	4	350	4	3
44822	2	262	2	3	52064	2	257	2	3
45060	3	388	3	4	52533	8	749	8	7
45231	5	552	5	6	52565	8	750	8	7
45238	3	250	3	2	52587	8	749	8	7
45474	5	449	5	4	52620	8	750	8	7
45549	8	748	8	7	52792	3	354	3	4
45571	8	750	8	7	52823	7	650	7	6
45697	7	649	7	6	52986	2	150	2	1
45725	2	148	2	1	53050	3	237	3	2
45755	6	687	6	7	53647	8	750	8	7
45890	5	695	5	7	53868	5	449	5	4
46073	9	850	9	8	54026	3	250	3	2
47162	0	949	40	39	54145	1	050	1	0
47476	1	169	1	2	54419	0	951	70	69
48305	5	445	5	4	54586	8	866	8	9
48359	9	648	9	6	54708	3	250	3	2
48614	6	653	6	7	54825	4	350	4	3
48626	8	748	8	7	55010	0	950	50	49
48807	6	875	6	9	55115	8	750	8	7
48845	0	939	40	39	55313	9	850	9	8
48980	9	789	9	8	55996	8	864	8	9
49047	6	550	6	5	57089	8	750	8	7
49409	1	194	1	2	57202	7	649	7	6
50211	9	850	9	8	57486	6	550	6	5
50414	1	050	1	0	57751	8	750	8	7

Number.	Vlacq.	Thomson.	Lefort. Err.	Lefort. Corr.	Number.	Vlacq.	Thomson.	Lefort. Err.	Lefort. Corr.
58081	2	150	2	1	67399	0	949	30	29
58214	6	550	6	5	67686	9	608	9	6
58223	2	149	2	1	69311	7	773	7	8
58301	1	000	1	0	69457	3	250	3	2
58858	7	649	7	6	69477	5	450	5	4
59007	1	050	1	0	69988	2	150	2	1
59482	9	647	9	6	70019	0	949	40	39
59498	4	349	4	3	70040	3	411	3	4
60096	2	250	2	3	70043	1	004	1	0
60401	8	868	8	9	70066	7	750	7	8
60487	2	149	2	1	70599	6	528	6	5
60704	1	200	1	2	71140	9	850	9	8
61011	4	349	4	3	71306	9	834	9	8
61157	4	349	4	3	71518	5	918	5	9
61606	4	515	4	5	71569	0	064	0	1
62038	5	449	5	4	71653	3	351	3	4
62131	7	650	7	6	71764	6	526	6	5
62173	7	650	7	6	72103	9	849	9	8
62257	4	350	4	3	72675	5	442	5	4
62273	4	349	4	3	73046	0	950	90	89
62412	9	822	9	8	73059	4	482	4	5
62933	0	948	50	49	73286	2	251	2	3
63183	9	850	9	8	73303	0	950	90	89
63357	0	909	50	49	73404	6	545	6	5
63887	1	050	1	0	73501	9	812	9	8
64086	5	450	5	4	73570	1	042	1	0
64639	1	050	1	0	73571	2	148	2	1
64661	4	811	4	8	73655	9	849	9	8
64993	0	949	40	39	74527	6	550	6	5
65143	1	049	1	0	74723	8	749	8	7
65185	8	862	8	9	74733	5	450	5	4
65311	5	449	5	4	74932	5	449	5	4
65376	6	952	6	0	74941	0	949	40	39
65659	1	050	1	0	75149	9	850	9	8
65946	2	251	2	3	75386	2	150	2	1
66187	7	650	7	6	75395	6	546	6	5
66239	4	350	4	3	75560	3	250	3	2
66423	7	650	7	6	75562	4	453	4	5

Number.	Vlacq.	Thomson.	Lefort. Err.	Lefort. Corr.	Number.	Vlacq.	Thomson.	Lefort. Err.	Lefort. Corr.
75613	4	349	4	3	93155	1	173	1	2
75733	2	808	2	8	93202	7	878	7	9
75841	8	749	8	7	93498	0	052	0	1
75953	4	349	4	3	94468	7	922		9
77047	2	141	2	1	96453	7	151	7	2
77437	6	550	6	5	96981	0	943	80	79
77663	7	649	7	6	97361	2	907	2	9
77944	6	542	6	5	97674	5	552	5	6
78079	5	450	5	4	98336	5	576	5	6
78259	2	149	2	1	98337	9	988	49	50
79447	5	449	5	4	98338	3	355	3	4
79467	1	050	1	0	98339	6	677	6	7
79666	0	950	20	19	98340	9	954	39	40
80060	7	769	7	8	98341	1	186	1	2
80062	8	859	8	9	98342	3	373	3	4
80063	2	302	2	3	98345	6	666	6	7
80090	6	663	6	7	98346	6	674	6	7
80851	9	817	9	8	98348	5	554	5	6
81212	0	950	60	59	98350	2	255	2	3
81460	8	750	8	7	98352	7	777	7	8
82951	0	946	60	59	98353	4	470	4	5
82991	7	650	7	6	98356	2	281	2	3
83693	6	550	6	5	98357	7	794	7	8
83803	8	887	8	9	98358	2	264	2	3
85651	9	844	9	8	98359	6	688	6	7
85810	9	972	19	20	98360	0	067	0	1
86688	3	353	3	4	98362	6	690	6	7
86708	0	936	90	89	98365	2	289	2	3
86898	0	064	0	1	98366	3	399	3	4
87634	3	250	3	2	98367	4	464	4	5
89182		633	7	6	98772	8	736	8	7
89185		653	6	7	98936	7	780	7	8
90625	5	579	5	6	98966	4	339	4	3
91086	8	650	8	7	99926	2	115	2	1
91087	7	585	7	6					

A few words of explanation are required with regard to one or two numbers in this list. The last two figures of log 11,003 are in Vlacq 39, not 29, as given by Lefort, and the error may

have been merely a misprint of 9 for 0, or it may have been due to a misprint of 3 for 2, and an error of contraction. There is really no last-figure error in log 39,626, but the final 2 is imperfectly printed in some copies (see *Notices*, vol. xxxiii., pp. 453, 457, May 1873). The error in log 53,050 is assigned by Lefort to log 53,053, to which it could not belong. Mr. J. N. Lewis suggested (in the number of the *Notices* just cited) that the former logarithm was probably that which was meant; and as the correction is appropriate to it, and to no other in the vicinity—I had the last figures of the logarithms from 50,000 to 60,000 compared in Vlacq and Thomson—there can be no doubt that his explanation is the correct one.

Leaving out of consideration, for reasons already sufficiently explained, the cases where the last figures in the MS. are 50, there is only one instance in which Lefort's correction is not confirmed, viz. in log 54,419, where Thomson gives the eleventh and twelfth figures as 51. I accordingly calculated this logarithm *de novo* from the formula—

$$\log x = \frac{1}{2}\log(x-1) + \frac{1}{2}\log(x+1) + \frac{\text{modulus}}{2x^2-1} + \&c.$$

(54419 being a prime, $54418 = 2.7.13^2.23$, and $54420 = 10.2.3.907$), and found it to be ·73575 05569 49946 8 ... confirming the *Tables du Cadastre*. Of course, as we have seen before, Thomson's last figures are not to be trusted to a unit.*

* Since the account in the text was written Mr. Sang has sent me a copy of his paper *On Last-Place Errors in Vlacq's Table of Logarithms*, read before the Royal Society of Edinburgh, April 20, 1874, in which are given the results of a comparison between his own fifteen-figure MS. table (see § 4) and Vlacq for the ten chiliads 20,000–30,000. This comparison was made in entire ignorance of what was done in 1858 by Lefort, for Mr. Sang writes: "It is indeed surprising that after so many years we are still relying on the unchecked calculations of Briggs and Vlacq; that among so many generations of scientific men there has not been zeal enough to effect a revision of the canon." And, further, in speaking of the *Tables du Cadastre*: "I have not learned that these computations have been used for the verification of those already printed, or that they have served for the production of any seven-place table; and thus, up to the present moment, we have no verification of Vlacq's great work;" so that the list of 42 errors detected in Vlacq by means of his MS., which Mr. Sang gives in his paper, is quite independent of any previous work. On comparing it with Lefort's list in the text, I find that there are only four errata indicated which do not appear in the latter; but in three of these cases (viz. in log 24,580, log 26,517 and log 26,728) the corrections have previously been published. The error in log 24,580 was pointed out in vol. xxxii., p. 258, (May 1872) of the *Notices*, and that in log 26,517, although not noticed by Lefort, was given by Vega in 1794 (see *Notices*, vol. xxxii., p. 288, June 1872); log 26,728 is in Lefort's list, but is omitted from that in the text, as not being a last-figure error. The other erratum seems to have escaped M. Lefort. It occurs in log 26,699, where the last figure should be a 3 instead of a 4, the succeeding figures being 4903... : this is confirmed by Thomson, who gives the last two figures as 49.

The corrections to log 26,188, log 29,112, and log 29,163 do not appear in Mr. Sang's list; and although the middle one is confirmed by Thomson, in the other two the last two figures are 50. I accordingly calculated those logarithms

It is rather curious to observe in the above list how little superior in accuracy Vlacq's last myriad is to the others which he calculated himself, although, as it was given by Briggs to 14 places, he might have avoided all errors in it. Considering that all the last-figure errors made in 90,000 results are contained in the list, it is manifest that Vlacq was very careful to be accurate in the last figures, and it seems strange that he should not have ensured absolute freedom from error, where it would have cost him nothing to have done so.

In order to see how far M. Lefort's list might be relied upon as complete, I had, as is incidentally mentioned above, the last figure in Vlacq examined with Thomson from 50,000 to 60,000. The result was the discovery, besides the corrections given by Lefort, of the following three discrepancies:—

No.	Vlacq.	Thomson.
51,141	8	874
52,743	7	648
59,205	5	449

On calculating these three logarithms to 14 places I found the figures after the tenth to be 7326; 5029; 5120 respectively, so that the first discrepancy (viz. in log 51,141) is due to a last-figure error in Vlacq that has hitherto escaped detection; but the other two are attributable to last-figure errors in the MS.; the logarithms as given by Vlacq being correct. It is proper here to warn any user of Mr. Thomson's MS. against his 3's, which, though very carefully made, are yet in cases very likely to be taken as 2's. Thus the antepenultimate figure in log 55,059 is either an excellent 3 or an excellent 2, according as it first strikes the eye. Other examples will be found in the antepenultimate figure of log 58,208 and the last figure but three of log 101,297. Several apparent discrepancies I found to be due to this cause.

§ 4. *Other Logarithmic MSS.—Publication of Mr. Thomson's MS.*

It still remains to notice some other unpublished logarithmic MSS. First in importance and celebrity stand the *Tables du*

to 14 places, and (taking the values of logs 6547, 7278, 9721 from Briggs) found the figures after the tenth to be 4995; 7818; 4997, confirming Mr. Sang in the first and third cases, and the *Tables du Cadastre* in the other, which probably inadvertently escaped Mr. Sang in the reading. M. Lefort states that the last two figures of the *Tables du Cadastre* are not to be relied upon; and this renders all his corrections where the last two figures in Thomson are 50 uncertain. The only cases where Thomson's last figure, as given in the list in the text, differs from the truth, between 20,000 and 30,000, are in 21,749, 21,795, 22,877, 24,156, 29,226, where the final three figures, according to Mr. Sang, should be 269; 354; 181; 936; 955. On the whole, however, it is satisfactory to obtain so good a confirmation of the general accuracy of M. Lefort's list in all essential points; and when comparing it with Mr. Thomson's MS. I little thought that further testimony as to its correctness was so soon to be added.

Cadastre, which have been referred to already. There is, however, also another MS., giving the logarithms of numbers above 100,000, which was calculated by the late Mr. G. H. Heppel, an actuary, who died in 1845. His tables contain the logarithms of numbers from 100,000 to 118,600 (to twelve places), and from 131,000 to 136,636, and 200,000 to 210,000 (to twelve or fifteen places). The first portion (100,000 to 118,600) *has been stereotyped*,* and the plates and MSS., Mr. Merrifield tells me, are now in the possession of Mr. C. Jellicoe, late President of the Institute of Actuaries. A seven-figure table of logarithms of numbers to 250,000 was calculated by Cartault or Cartaud, a Commis de la Marine, who died in 1784; in that year it passed into the hands of Lalande, as also did the table of logarithmic sines and tangents to every second which was calculated by Robert, Curé of Toul (see Lalande, *Bibl. Astron.*, pp. 667 and 688; Delambre, *Rapport Historique sur les Progrès des Sciences Mathématiques depuis* 1789 (1810), p. 64; and *Encyc. Méthod.* Art. *Tables Astronomiques*). Both passed into the hands of Delambre, and the former (viz. the logarithms of numbers) is now in the Graves Library, at University College, London. The sines and tangents were bought by Babbage, and are now in the possession of Lord Lindsay. There are in our own (the Astronomical Society's) library two volumes of MSS., the first of which relates to logarithms of numbers; they were calculated by the late Mr. T. W. Hill, F.R.A.S., and presented to the Society by his son, Mr. M. D. Hill, Q.C. The logarithms of numbers were intended, as appears from the title, to extend as far as 220,000 to 23 places of decimals (21 or 22 correct), and were commenced in the year 1806, the author's desire being to excel the *Tables du Cadastre*. The logarithmic work, although it fills a quarto work of moderate thickness, is far too incomplete to be of much value. To take an example: On one page we have log 849 to 23 places, then to this log 2 is added eight times successively, giving the logarithms of 1698, 3396, . . . 217344; 851 and 853 are similarly treated. On another page further on log 3 is repeatedly added to the same three logarithms. The highest "fundamental" number is 1231, so that only the logarithms of multiples of numbers less than 1231 by powers of 2 and 3 are given; and any of these logarithms could easily be calculated *de novo* by a computer who required them. At the end of the volume is a factor table up to about 11,000. Had Mr. Thomson written down each time, as Mr. Hill has done, the logarithms he added (instead of using moveable slips), his MS. would have been of very great bulk. In the *Edinburgh Transactions*, vol. xxvi. (1872), Mr. Sang has described the manner in which he calculated a fifteen-figure table of the logarithms of numbers from 100,000 to 200,000, the first seven figures of which only he printed in his "New table of seven-place logarithms" (1871). He has been

* *Assurance Magazine*, vol. x. p. 82 (1863).

subsequently extending the table with the view of publishing a nine-figure table of logarithms up to 1,000,000. The reader of Mr. Sang's account will see that his mode of calculation in some respects resembles that which was probably pursued by Mr. Thomson.*

There can be no doubt that besides these MSS. there must be many others of more or less extent in private hands. It is not very uncommon for persons with a taste for mathematics to devote their leisure to the formation of logarithmic tables, though most likely there is no case in which so extensive a table as Mr. Thomson's has ever thus been produced. Either from the schemes proposed to themselves by their authors being too ambitious, or for other reasons, such tables are rarely completed, and would probably not be published, even if the necessary funds were forthcoming, as it is so much easier to calculate a table than to free from it errors when calculated. I have heard of two or three MSS. that have been constructed in this way. The celebrity attained by the French tables and the frequent expressions of regret in this country at their non-publication, coupled with the fact that the English Government once offered 5,000*l.* towards the printing, have no doubt several times, as was the case with Mr. Hill, incited computers to attempt to equal or surpass them, at least as far as the logarithms of numbers were concerned. But with regard to the *Tables du Cadastre*, it ought to be remembered that the logarithms of numbers only form a small portion of the whole MSS., and that the publication is, or rather was, urged on the ground, that the French Government, having undertaken the calculation, should complete its work by giving the results to the world. Were it a question of performing the whole work over again, it is very questionable whether many would be inclined to recommend a Government to undertake it at all.

The same causes that have prevented the publication of the *Tables du Cadastre* will probably prevent the publication of Mr. Thomson's MS. Such a table if printed would be very little used; and besides, anyone who desired to publish the whole would have to devote so much attention to verifications before he could be satisfied of its complete accuracy, that the work would not fall very far short of what would be required for re-computing all the portion not previously printed. No public body could well give funds towards the publication, as it is unquestionable that the money could be expended much more usefully, even in the matter of mathematical tables. It thus appears that until a ten-figure table to 120,000—a work really wanted, and one which, on account of its practical value and the public use that might be made of it, would be a proper devotion of national funds—no further use can be made of Mr. Thomson's MS., except for the detection of un-

* In the paper referred to in the note to § 3 Mr. Sang states that he has completed his fifteen-figure table up to 320,000, and that this portion, occupying 24 quarto volumes, was exhibited to the Royal Society of Edinburgh on April 20.

discovered errors in Vlacq. In this view I am supported by Mr. C. W. Merrifield, who writes to me: "Taking it for granted that Vega is unprocurable practically, what is chiefly needed is a republication of his work. I should be disposed to add 19 chiliads of common logarithms 101,000–120,000.... I think the question of very advanced logarithms, such as twelve-figure, lands us on a different step. For the present state of science I think an accessible reprint of the ten-figure table, circular as well as numerical, is required. The other is a desirable luxury, but only a luxury. Unless I can get it, I have no more right to a fourteen or twenty-figure table, because one exists, than I should have to *claim* the use of the 'Great Eastern,' because I wanted to try experiments on ships exceeding 600 feet in length. I attach importance to the 101st-120th chiliads. The second difference in ten figures only affects the last figure to the extent of 4 units at 120,000, and the break at 100,000 is a bad stopping-place, having regard to reciprocals. At the same time I do not think differences the proper mode of interpolation for any well-known and simple function like logarithms or circular functions. Adaptations of Taylor's theorem should always be used, except where proportional parts will do; but it is desirable to push the use of these without second differences as far as practicable, and for this the extension to 120,000 is very desirable."

Although computers would be very glad to have a twelve-figure table to refer to, it would be, as Mr. Merrifield observes, a luxury, and only a luxury, so that the MS. is not likely to be printed as it stands. I should recommend that the books containing the final results, viz. A_2, A_3, A_4, be bound together in one volume. A_1 being of smaller size, must be kept separate. The other volumes, containing the differences and the work, are of different sizes; and it would perhaps be desirable to let them remain as they are, so that anyone interested in the subject may be able to see them exactly as they were when in Mr. Thomson's possession.

It will have been seen that no printed table and only two MSS. (the *Tables du Cadastre* and Mr. Sang's) are more extensive than Mr. Thomson's table, which will henceforth be generally available for reference. Even had Mr. Thomson lived to complete his work, it is scarcely likely that it could have been put to any other use, besides that to which it has been applied in this account, viz. the verification of existing tables; and the thanks of all computers are due to Miss Thomson for having, by her valuable present, made so important a contribution towards the accuracy of logarithmic tables.

On the Combination of the different Results of various Series of Observations. By Dr. C. Powalky.

1. *Direct solution* for two series.

Let $[v_1]$ be the sum of all values v_1 of the first series; n_1 the number of its observations; $[d_1 d_1]$ the sum of the squares, $v_1 - \frac{[v_1]}{n_1}$;

Let $[v_2]$ be the sum of all values v_2 of the second series; n_2 the number of its observations; $[d_2 d_2]$ the sum of the squares, $v_2 - \frac{[v_2]}{n_2}$;

p_1, p_2 the weights.

The general equation is:

$$w = \frac{p_1[v_1]+p_2[v_2]}{p_1 n_1 + p_2 n_2} = \frac{[v_1]}{n_1} - \frac{\left(\frac{[v_1]}{n_1} - \frac{[v_2]}{n_2}\right) p_2 n_2}{p_1 n_1 + p_2 n_2} = \frac{[v_2]}{n_2} + \frac{\left(\frac{[v_1]}{n_1} - \frac{[v_2]}{n_2}\right) p_1 n_1}{p_1 n_1 + p_2 n_2}.$$

Putting now

$$\frac{[v_1]}{n_1} - \frac{[v_2]}{n_2} = \mathrm{D}; \; p_1 n_1 + p_2 n_2 = \mathrm{Z},$$

the squares of the errors are

$$[d_1 d_1] + n_1 \frac{\mathrm{D}^2 n_2^2 p_2^2}{\mathrm{Z}^2}; \; [d_2 d_2] + n_2 \frac{\mathrm{D}^2 n_1^2 p_1^2}{\mathrm{Z}^2};$$

and therefore

$$p_1 : p_2 = \frac{[d_2 d_2] + n_2 \frac{\mathrm{D}^2 n_1^2 p_1^2}{\mathrm{Z}^2}}{n_2 - 1} : \frac{[d_1 d_1] + n_1 \frac{\mathrm{D}^2 n_2^2 p_2^2}{\mathrm{Z}^2}}{n_1 - 1}$$

$$= (n_1 - 1)([d_2 d_2]\mathrm{Z}^2 + n_2 n_1^2 \mathrm{D}^2 p_1^2) : (n_2 - 1)([d_1 d_1]\mathrm{Z}^2 + n_1 n_2^2 \mathrm{D}^2 p_2^2),$$

or

$$(n_2 - 1) p_1 \{\mathrm{Z}^2 [d_1 d_1] + n_1 n_2^2 \mathrm{D}^2 p_2\} = (n_1 - 1) p_2 \{\mathrm{Z}^2 [d_2 d_2] + n_1^2 n_2 \mathrm{D}^2 p_1\}.$$

Taking $p_1 = q p_2$, we have

$$\mathrm{Z}^2 = p_2^2 (q n_1 + n_2)^2, \text{ and dividing by } p_2^3$$

$$(n_2 - 1) q \{(q n_1 + n_2)^2 [d_1 d_1] + n_1 n_2^2 \mathrm{D}^2\} = (n_1 - 1)\{(q n_1 + n_2)^2 [d_2 d_2] + n_1^2 n_2 \mathrm{D}^2\};$$

or

$$\alpha q^3 + \beta q^2 + \gamma q + \delta = 0.$$

If the problem is determinate, we find only one real value of q

from this equation, the other two being imaginary; but the problem is indeterminate if there are three real values.

2. The *indirect solution* is more practicable, and applicable to any number of series.

Take w as known, we have for the determination of $p_1, p_2, p_3 \ldots$

$$\frac{1}{p_1} = \left\{\left(w - \frac{[v_1]}{n_1}\right)^2 n_1 + [d_1 d_1]\right\} \div (n_1 - 1)$$

$$\frac{1}{p_2} = \left\{\left(w - \frac{[v_2]}{n_2}\right)^2 n_2 + [d_2 d_2]\right\} \div (n_2 - 1)$$

$$\frac{1}{p_3} = \left\{\left(w - \frac{[v_3]}{n_3}\right)^2 n_3 + [d_3 d_3]\right\} \div (n_3 - 1).$$

$$\vdots \qquad\qquad \vdots$$

Assuming for w any value for w', determine the weights for this, and determine—if w' is not right—another value w''; assuming now $w = w''$, determine a new value w''', and so on until $w^{(n+1)} = w^{(n)}$.

Example.

$\frac{[v_1]}{n_1} = +12{\cdot}8, n_1 = 9$; $\frac{[v_2]}{n_2} = +1{\cdot}5, n_2 = 4$; $\frac{[v_3]}{n_3} = +1{\cdot}6, n_3 = 5$; $\frac{[v_4]}{n_4} = +0{\cdot}5, n_4 = 7$.

$[d_1 d_1] = 40{\cdot}0$ $\quad [d_2 d_2] = 1{\cdot}1$ $\quad [d_3 d_3] = 12{\cdot}3$ $\quad [d_4 d_4] = 12{\cdot}0$.

1. $w' = +6{\cdot}0$.

n	9	4	5	7
$w - \frac{[v]}{n}$ . . .	−6·8	+4·5	+4·4	+5·5
Squares . . .	46·24	20·25	19·37	30·26
Multip. by n . .	416·2	81·0	96·9	211·8
$+[dd]$ . . .	40·0	1·1	12·3	12·0
Sum . . .	456·2	82·1	109·2	223·8
$\div$ by $(n-1)$ . .	57·0	27·4	27·3	37·3
$\log \frac{1}{p} =$ . . .	1·7559	1·4378	1·4362	1·5717
$\log [v]$ . . .	2·0615	0·7782	0·9031	0·5441
$\log p$	8·2441	8·5622	8·5638	8·4283
$\log n$	0·9542	0·6021	0·6990	0·8451

$[v]p$	np
+2·021	0·158
+0·219	0·146
+0·293	0·183
+0·094	0·188
+2·627	0·675

$w'' = +3{\cdot}9$

$$w'' = \frac{[v_1]p_1 + [v_2]p_2 + [v_3]p_3 + [v_4]p_4}{n_1 p_1 + n_2 p_2 + n_3 p_3 + n_4 p_4}$$

2. $w'' = +3{\cdot}9$.

n	9	4	5	7
$w - \frac{[v]}{n}$	$-8{\cdot}9$	$+2{\cdot}4$	$+2{\cdot}3$	$+3{\cdot}9$
Squares	79·22	5·76	5·29	11·56
Multip. by n	713·0	23·0	26·5	80·9
$+[d\,d]$	40·0	1·1	12·3	12·0
Sum	753·0	24·1	38·8	92·9
$\div$ by $(n-1)$	94·3	8·07	9·7	15·5
$\log\frac{1}{p} =$	1·9743	0·9049	0·9868	1·1903
$\log[v]$	2.0615	0·7782	0·9831	0·5441
$\log p$	8·0257	9·0951	9·0132	8·8097
$\log n$	0·9542	0·6021	0·6990	0·8451

$[v]p$	np
$+1{\cdot}222$	0·096
$+0{\cdot}747$	0·498
$+0{\cdot}825$	0·516
$+0{\cdot}226$	0·451
$+3{\cdot}020$	1·561

$w''' = +1{\cdot}93$

3. $w''' = +1{\cdot}93$.

$w - \frac{[v]}{n} = Q$	$-10{\cdot}87$	$+0{\cdot}43$	$+0{\cdot}33$	$+1{\cdot}43$
Q^2	118·1	0·185	0·109	2·045
$n\,Q^2$	1062·9	0·74	0·54	14·3
$+[d\,d]$	40·0	1·1	12·3	12·0
Sum = S	1102·9	1·84	12·84	26·3
$\frac{S}{n-1}$	137·9	0·6	3·2	4·6
$\log p =$	7·8604	0·2218	9·4949	9·3372

$[v]p$	np
$+\ 0{\cdot}835$	0·065
$+10{\cdot}000$	6·667
$+\ 2{\cdot}500$	1·563
$+\ 0{\cdot}760$	1·522
$+14{\cdot}095$	9·817

$w^{(4)} = +1{\cdot}44$

4. $w^{(4)} = +1{\cdot}4$.

$w - \frac{[v]}{n} = Q$	$-11{\cdot}4$	$-0{\cdot}1$	$-0{\cdot}2$	$+0{\cdot}9$
Q^2	130·0	0·01	0·04	0·81
$n\,Q^2$	1170·0	0·04	0·20	5·67
$+[d\,d]$	40·0	1·1	12·3	12·0
Sum = S	1210·0	1·14	12·5	17·67
$\frac{S}{n-1}$	151·0	0·38	3·18	2·94
$\log p =$	7·8210	0·4318	9·5086	9·5229

$[v]p$	np
$+\ 0{\cdot}76$	0·06
$+16{\cdot}22$	10·81
$+\ 2{\cdot}58$	1·61
$+\ 1{\cdot}17$	2·33
$+20{\cdot}74$	24·81

$w^{(5)} = +1{\cdot}40$

$p_1 = 0{\cdot}007$
$p_2 = 2{\cdot}703$
$p_3 = 0{\cdot}323$
$p_4 = 0{\cdot}333$

$w = +1{\cdot}40$ is here but one of the values; for, assuming first $w' = +11{\cdot}0$, we find $w'' = +11{\cdot}6$, $w''' = +11{\cdot}9$, and $w^{(4)} = w^{(5)} = +12{\cdot}0$ with the weights: $p_1 = 0{\cdot}170$, $p_2 = 0{\cdot}007$, $p_4 = 0{\cdot}063$. A third value of w will be found $= +9{\cdot}0$, and the weights $p_1 = 0{\cdot}047$, $p_2 = 0{\cdot}013$, $p_3 = 0{\cdot}014$, $p_4 = 0{\cdot}012$. The problem is indeterminate.

Excluding the first value of $\frac{[v]}{n}$ we obtain $w = +1{\cdot}38$, and no other value satisfying the conditions

$$w = \frac{p_2[v_2] + p_3[v_3] + p_4[v_4]}{n_2 p_2 + n_3 p_3 + n_4 p_4},$$

the weights are now:

$$p_2 = 3{\cdot}7,\ p_3 = 0{\cdot}32,\ p_4 = 0{\cdot}34.$$

The problem is determinate.

Washington, 1874, *May.*

Remark on the Influence of Errors of Observation on the Determination of the Orbit of a Planet from three Observations.

By Herr F. W. Berg.

(*Translation.*)

Let a be the semiaxis major of the orbit, e ($= \sin\phi$) the excentricity, c the mean anomaly of the epoch, ϖ the distance of perihelion from ascending node, Ω the longitude of ascending node, i the inclination to the ecliptic; and moreover let α and β be the longitude and latitude for the first observation, α' and β' for the second, and α'' and β'' for the third. Then, as is known, compare Gauss, *Theoria Motus*, Art. 76–76:

$$\begin{aligned} d\alpha &= \mathrm{A}\,da + \mathrm{E}\,de + \mathrm{C}\,dc + \mathrm{W}\,d\varpi + \mathrm{I}\,di + \mathrm{O}\,d\Omega, \\ d\beta &= \mathrm{a}\,da + \mathrm{e}\,de + \mathrm{c}\,dc + \mathrm{w}\,d\varpi + \mathrm{i}\,di + \mathrm{o}\,d\Omega, \end{aligned} \qquad (1)$$

and similarly for the other two observations.

The coefficients A, E, &c., depend on the time t and the elements a, e, &c., of the orbit, and we have thus in these equations the relation between the variations of α and β and those of the elements. From the equations (1) and the corresponding equations for the other two observations can be obtained new equations serving to express the variations of the elements in terms of the variations $d\alpha$, $d\beta$, $d\alpha'$, $d\beta'$, $d\alpha''$, $d\beta''$. We obtain

$$\begin{aligned} \Delta\, da &= (\alpha, a)\, d\alpha + (\alpha', a)\, d\alpha' + (\alpha'', a)\, d\alpha'' + (\beta, a)\, d\beta + (\beta', a)\, d\beta' + (\beta'', a)\, d\beta'', \\ \Delta\, de &= (\alpha, e)\, d\alpha + (\alpha', e)\, d\alpha' + (\alpha'', e)\, d\alpha'' + (\beta, e)\, d\beta + (\beta', e)\, d\beta' + (\beta'', e)\, d\beta'. \end{aligned} \qquad (2)$$

&c.

Considering now the quantities $d\alpha$, $d\beta$, &c., as errors of

observation, these it is clear will exercise the greatest influence on the values of the elements when the determinant Δ vanishes. To investigate when this happens: the value of the determinant is

$$\Delta = \begin{vmatrix} \mathrm{A\,a} & \mathrm{A'\,a'} & \mathrm{A''\,a''} \\ \mathrm{E\,e} & \mathrm{E'\,e'} & \mathrm{E''\,e''} \\ \mathrm{C\,c} & \mathrm{C'\,c'} & \mathrm{C''\,c''} \\ \mathrm{W\,w} & \mathrm{W'w'} & \mathrm{W''w''} \\ \mathrm{I\,i} & \mathrm{I'\,i'} & \mathrm{I''\,i''} \\ \mathrm{O\,o} & \mathrm{O'\,o'} & \mathrm{O''\,o''} \end{vmatrix}. \tag{3}$$

Let x, y, z be, for the first observation, the rectangular heliocentric co-ordinates of the planet; X, Y, Z those of the Earth; and ρ the distance of the Earth and planet; then

$$\begin{aligned} x &= \rho \cos\beta \cos\alpha + \mathrm{X}, \\ y &= \rho \cos\beta \sin\alpha + \mathrm{Y}, \\ z &= \rho \sin\beta \qquad\quad + \mathrm{Z}, \end{aligned} \tag{4}$$

and similarly for the second and third observations.

Then X, Y, Z being regarded as constant; we have (compare Gauss, *Theoria Motus*, Art. 76):

$$\begin{aligned} d\alpha &= \mathrm{o}\,dz + \frac{\cos\alpha}{\rho\cos\beta}\,dy - \frac{\sin\alpha}{\rho\cos\beta}\,dx, \\ d\beta &= \frac{\cos\beta}{\rho}\,dz - \frac{\sin\alpha\sin\beta}{\rho}\,dy - \frac{\sin\beta\cos\alpha}{\rho}\,dx. \end{aligned} \tag{5}$$

If now v be the true anomaly of the planet, and r its radius vector (both for the first observation) the co-ordinates x, y, z can also be expressed as follows:

$$\begin{aligned} x &= r\{\cos(v+\varpi)\cos\Omega - \sin(v+\varpi)\sin\Omega\cos i\}, \\ y &= r\{\cos(v+\varpi)\sin\Omega + \cos(v+\varpi)\cos\Omega\cos i\}, \\ z &= r.\sin(v+\varpi)\sin i; \end{aligned} \tag{6}$$

and from these at once

$$\begin{aligned} \frac{dx}{d\Omega} &= -y, & \frac{dx}{di} &= z\sin\Omega, \\ \frac{dy}{d\Omega} &= x, & \frac{dy}{di} &= -z\cos\Omega, \\ \frac{dz}{d\Omega} &= \mathrm{o}, & \frac{dz}{di} &= z\cos z; \end{aligned} \tag{7}$$

$$\begin{aligned} \frac{dx}{d\varpi} &= -y\cos i - x\cos\Omega\sin i, \\ \frac{dy}{d\varpi} &= x\cos i - z\sin\Omega\sin i, \\ \frac{dz}{d\varpi} &= (x\cos\Omega + y\sin\Omega)\sin i; \end{aligned} \tag{8}$$

$$\frac{dx}{dc} = \frac{a}{\cos\phi}\left\{\frac{p}{r^2}\frac{dx}{d\varpi} + e\sin v\frac{x}{r}\right\},$$
$$\frac{dx}{da} = \frac{x}{a} - \frac{3mt}{a}\frac{dx}{dc}, \qquad (9)$$
$$\frac{dx}{de} = -a\frac{x}{r}\cos v + \frac{p+r}{\cos^2\phi}\frac{\sin v}{r}\frac{dx}{d\varpi},$$

where m is the mean daily motion of the planet, and t is the time; also $p = a\cos^2\phi, = a\,(1-e^2)$. In the last three equations (9), x may be changed into y, or into z. In the equations (6), (7), (8), and (9), we may for x, y, z, write x', y', z', or x'', y'', z'', changing r, v, t into r', v', t', r'', v'', t'' accordingly.

From these partial variations of the co-ordinates in respect to the elements, the total variations may be deduced; we have

$$dx = \frac{dx}{da}da + \frac{dx}{de}de + \frac{dx}{dc}dc + \frac{dx}{d\varpi}d\varpi + \frac{da}{di}di + \frac{dx}{d\Omega}d\Omega. \qquad (10)$$

&c.

Introducing this and the like expressions for y, z, x', y', z', x'', y'', z'', into the expressions (5), we obtain the variations $d\alpha$, $d\beta$, $d\alpha'$, &c., expressed in the terms of the variations da, de, dc, &c., as the equations (1) require.

The formation of the values of the coefficients A, a, &c., is also easy. We have, for example:

$$A = \frac{1}{\rho}\left\{\frac{\cos\alpha}{\cos\beta}\frac{dy}{da} - \frac{\sin\alpha}{\cos\beta}\frac{dx}{da}\right\},$$
$$A' = \frac{1}{\rho'}\left\{\frac{\cos\alpha'}{\cos\beta'}\frac{dy'}{da} - \frac{\sin\alpha'}{\cos\beta'}\frac{dx'}{da}\right\}. \qquad (11)$$

&c.

In order to make the determinants more manageable, the terms may be collected as follows; write

$$\begin{vmatrix} A, & a \\ E, & e \end{vmatrix} = P, \quad \begin{vmatrix} C, & c \\ W, & w \end{vmatrix} = Q, \quad \begin{vmatrix} I, & i \\ O, & o \end{vmatrix} = T; \qquad (12)$$

and similarly for P′, P″, &c.; viz. $P = Ae - Ea$, &c.; then the determinant may be written:

$$\Delta = \begin{vmatrix} P, & P', & P'' \\ Q, & Q', & Q'' \\ T, & T', & T'' \end{vmatrix}. \qquad (13)$$

We hence obtain

$$\Delta = L\begin{vmatrix} M\frac{R}{\rho}\sin(A-\Omega), & M'\frac{R'}{\rho'}\sin(A'-\Omega), & M''\frac{R''}{\rho''}\sin(A''-\Omega) \\ N\frac{R}{\rho}\sin(A-\Omega), & N'\frac{R'}{\rho'}\sin(A'-\Omega), & N''\frac{R''}{\rho''}\sin(A''-\Omega) \\ \frac{\sin u\cos\psi}{\cos\beta}, & \frac{\sin u'\cos\psi'}{\cos\beta'}, & \frac{\sin u''\cos\psi''}{\cos\beta''}; \end{vmatrix} \qquad (14)$$

where

$$
\begin{aligned}
\mathrm{M} &= \frac{p+r}{p\,r}\sin v - \frac{3\,a\,m\,t}{2\,r^2\cos\psi}(e+\cos\epsilon) \\
\mathrm{M}' &= \frac{p+r'}{p\,r'}\sin v' - \frac{3\,a\,m\,t'}{2\,r'^2\cos\psi'}(e+\cos\epsilon') \\
\mathrm{M}'' &= \frac{p+r''}{p\,r''}\sin v'' - \frac{3\,a\,m\,t''}{2\,r''^2\cos\psi''}(e+\cos\epsilon'') \qquad (15) \\
\mathrm{N} &= \frac{a\,e\sin v}{r\cos\psi}, \quad \mathrm{N}' = \frac{a\,e\sin v'}{r'\cos\psi'}, \quad \mathrm{N}'' = \frac{a\,e\sin v''}{r''\cos\psi''} \\
\mathrm{L} &= -\left(\frac{r\,r'\,r''}{\rho\,\rho'\,\rho''}\right)^2 \sin^2 i\cos i;
\end{aligned}
$$

and ϵ, ϵ', ϵ'' are the excentric anomalies, and u, u', u'' the arguments of latitude of the planet, R, R′, R″, the radius vectors of the Earth, A, A′, A″ its longitudes, and ψ, ψ', ψ'' the angles at the planet (in the triangle Sun, Planet, Earth), which, as is known, Gauss first introduced into the problem.

From the expression (14) it appears that the determinant Δ vanishes when

(1.) The planet in one of the three observations is at once in the ecliptic, and in opposition with the Sun, for then the corresponding A is $=\Omega$, and the corresponding $u=0$.

(2.) $i=0$, or $=\frac{\pi}{2}$.

(3.) $\psi=\psi'=\psi''=\frac{\pi}{2}$: in such a case the passage from ρ to r is very uncertain.

(4.) $\mathrm{MN}'-\mathrm{M}'\mathrm{N}=\mathrm{M}'\mathrm{N}''-\mathrm{M}''\mathrm{N}'=\mathrm{M}''\mathrm{N}-\mathrm{MN}''=0$: one of these three equations being always a consequence of the other two. From the conditions of this case relations may be obtained between the elements of the orbit, and the intervals θ, θ'', which may be introduced $t=t'-\theta$, $t''=t'+\theta''$.

(5.) $e=0$; viz., when the orbit is circular.

If the three positions of the planet and those of the Sun lie in one and the same great circle, the determinant is then very small; in such a case the difference between the longitudes of the Sun's positions and that of the node Ω is also very small, and the planet is then usually in opposition.

Observatory, Wilna, 1874, *May* 15/3.

Note on the Comparison of a Star in the Radcliffe Catalogue with others in the same Catalogue. By M. Antoine d'Abbadie.

Having undertaken last autumn to determine my colatitude by measuring, with a micrometer, zenith distances of stars at their passage through a fixed telescope placed vertically, I found on comparing No. 6100 of the Radcliffe Catalogue with four other stars of the same list and observed in the course of four different nights, that its N.P.D. was too small by a little less than 6″. Not presuming to set up my own authority against that of such an eminent observer as the late Radcliffe Astronomer, I requested Mons. Leverrier to get R. 6100 observed, stating merely that found its N.P.D. wrong. Three observations obtained at the Paris Observatory confirmed my result to a fraction of a second.

Although every observer is liable to mistakes, I am still diffident in this case, for the following reasons:—Johnson had four observations in N.P.D. If, as may be supposed, he reduced immediately one or two of them, and compared the result with that of Groombridge, he can scarcely have made, four times, the same mistake of 6″·3. Again, it is to be remarked that Groombridge obtained 46° 46′ 58″·8 in 1811·9, that 34·9 years later Johnson found 6″·3 *less*, and finally that in 1873·8, or 27 years later, M. Leverrier and myself agree in obtaining 5″·7 *more* than Johnson. This seems to indicate a period of slow change amounting to about 30 years. I would, therefore, recommend the observation of this star in N.P.D. to those who have suitable instruments. In Paris its R.A. has been found only 0ˢ·03 greater than that of Johnson.

On seeing the foregoing statement (communicated a the Meeting of June 1874) respecting the North Polar Distance of the star, No. 6100 of the Radcliffe Catalogue (identical with Groombridge 4083), Mr. Main immediately referred to the original computations, and found that an error of a very obvious character had been committed in taking the mean of two observations made in 1844, and two made in 1848, five seconds (the half of ten) having been omitted in the process. Such mistakes are not unfrequent, and arise from a desire to shorten the process of addition by not adding up the second column of figures. Still it is strange that the error was not detected in reading for the press, especially as the difference between this observed N.P.D. and that of Groombridge ought to have led to scrutiny. The printing was completed before Mr. Main took charge of the Observatory.

The erratum will stand as follows:—First Radcliffe Catalogue, page 351. No. 6100, Mean N.P.D. 1845·0, *for* 52″·5 *read* 57″·5.

Note on Sirius. By J. M. Wilson, Esq.

It may be worth while to point out that observations on *Sirius* as a double star are tending to confirm the result of spectroscopic examination of that star, as showing that its mass bears a less ratio to the Sun's mass than its brightness bears to the Sun's brightness; or, in other words, that it is intrinsically much brighter than the Sun, and therefore at a higher temperature.

The companion is not often within the reach of an $8\frac{1}{4}$-inch glass, but on one hazy evening last winter it occurred to me to turn the telescope to *Sirius*, and I saw one of the companions at once. Mr. Seabroke's measures agreed with my own, giving its position-angle 65°, and distance 11″. The measures were taken with a parallel-wire micrometer, without any contrivance for shutting off the light of the principal star, and with a power of about 400.

Mr. Gledhill has been good enough to furnish me with a few previously observed positions and distances of this companion:

	°		″		″
1863	88	and	7·6		
1865	76	„	10·0	to	8
1866	71	„	10·0	„	9

Both the positions and distances, so far as they may be considered accurate, show that the companion is passing away from its periastre, and they are insufficient to determine with any approach to accuracy either the period or the mean distance; but taking 200 years and 11″ as a very rough guess, founded on these measures, and taking the parallax of *Sirius* as 0″·22, it follows that the distance of the companion of *Sirius* is 50 times the Earth's distance from the Sun, and, therefore, that the mass of *Sirius*

$$=\frac{50^3}{200^2}\times\text{mass of Sun}=3\tfrac{1}{8}\text{ mass of Sun.}$$

But the amount of light given by *Sirius* has been estimated as more than 200 times that of the Sun, from which it would seem to follow that, even allowing for large errors in the elements above assumed, *Sirius* is much brighter intrinsically than the Sun, and, therefore, as the spectroscope tells us, at a higher temperature.

I venture to bring this Note before the Society in its very incomplete state, in order to attract attention to the companion of *Sirius*, and to show that it is within the reach of moderate instrumental power.

Temple Observatory, Rugby,
1874, *June* 9.

Observations, taken at Churt, of the Value of Colatitude given by η Ursæ Majoris on the Prime Vertical.

By R. C. Carrington, Esq., F.R.S.

Knowing that the *Nautical Almanac* position of η *Ursæ Majoris* was closely in accordance with numerous observations in the twelve-year and seven-year Greenwich Catalogues, I thought that it would give me a good determination of latitude, if observed in the prime vertical. I had had no previous trial of this method, but my altazimuth is perfectly suited for observation in this position, as in any other. On the 25th of April, 1874, I found that the reading of coincidence of the middle wire, with its image in a trough of mercury, illuminated by a Bohnenberger eye-piece, was

1. W. at Azim. $173^{\circ}\ 10'$	2. N. $263^{\circ}\ 10'$	3. E. $353^{\circ}\ 10'$	4. S. $83^{\circ}\ 10'$
$58^{\text{rev.}}\cdot3335$	$58^{\text{rev.}}\cdot3295$	$58^{\text{rev.}}\cdot3160$	$58^{\text{rev.}}\cdot3110$

from which I concluded the reading for collimation = 0 to be $58^{\text{rev.}}\cdot323$, and so set it. The value of the wire being $[1''\cdot42941]$ gave the following corrections for level:

W. $+0''\cdot24$	N. $+0''\cdot25$	E. $-0''\cdot24$	S. $-0''\cdot25$

The value of the wires which will be required below were found to be:

1	2	3	4	5	6	7
[1·63203]	[1·53121]	[1·38947]	[$-\infty$]	[1·38045]	[1·52808]	[1·63850]
$-42^{\text{s}}\cdot858$	$-33^{\text{s}}\cdot979$	$-24^{\text{s}}\cdot517$	$0^{\text{s}}\cdot000$	$+24^{\text{s}}\cdot013$	$+33^{\text{s}}\cdot735$	$+43^{\text{s}}\cdot501$

and the side wires of η *Urs. Maj.* were computed by the formula given by Encke in the *Berlin Jahrbuch*, for 1843:

$$t'-t = \frac{\text{wire}}{\sin\delta \,.\, \cos\phi \,.\, \sin t} - \frac{1}{2}\cot t \,.\, 15 \sin 1'' \left(\frac{\text{wire}}{\sin\delta \,.\, \cos\phi \,.\, \sin t}\right)^2 + \frac{1}{6}(1+3\cot^2 t)(15 \,.\, \sin 1'')^2 \left(\frac{\text{wire}}{\sin\delta \,.\, \cos\phi \,.\, \sin t}\right)^3;$$

where t for wire 1 in May 29, second observation is, for instance—

$$14^{\text{h}}\ 43^{\text{m}}\ 40^{\text{s}} - 13^{\text{h}}\ 42^{\text{m}}\ 31^{\text{s}}\cdot3 = 1^{\text{h}}\ 1^{\text{m}}\ 8^{\text{s}}\cdot7 = 15^{\circ}\ 17'\ 10''\cdot5,$$

w 1 is [1·63203], $l \sin t = 9\cdot42101$, $l \sin\delta = 9\cdot80858$, and $l \cos\phi = 9\cdot89132$.

The signs require attention; for instance, in the second observation of May 29

The first term of wires 1, 2, 3 is +, and for wires 5, 6, 7 −
" second " " " −, " " −
" third " " " +, " " −,

May 29. *η Ursæ Majoris*, observed at azimuth 263° 4′ 26″·35:

Wires.	h m s	Correction. m s	h m s
7	12 31 17·5	+4 53·60 =	12 36 11·10
6	12 32 21·6	+3 49·44 =	11·04
5	12 33 26·5	+2 44·60 =	11·10
4	12 36 10·5	0 0·00 =	10·50
3	12 39 6·3	−2 55·19 =	11·11
2	12 40 16·0	−4 4·96 =	11·04
1	12 41 23·5	−5 11·74 =	11·76
		Mean	12 36 11·09

Same star W. of meridian:

Wires.	h m s	Correction. m s	h m s
1	14 43 40·0	+5 11·67 =	14 48 51·67
2	14 44 48·5	+4 4·85 =	53·35
3	14 45 57·5	+2 55·14 =	52·64
4	14 48 52·1	0 0·00 =	52·10
5	14 51 36·3	−2 44·59 =	51·71
6	14 52 42·5	−3 49·35 =	53·15
7	14 53 46·1	−4 53·54 =	52·56
		Mean	14 48 52·45

β Libræ, observed at azimuth 173° 4′ 23″·25:

Wires.	h m s	Correction. s	h m s	D. Ab. s	Level. s	Azim. s	h m s
1	24·8	+43·38 =	8·18				
2	33·5	+34·39 =	7·89				
3	42·4	+24·82 =	7·22				
4	8·4	0·00 =	8·40				
5	32·6	−24·31 =	8·29				
6	42·2	−34·15 =	8·05				
7	15 10 52·0	−44·03 =	7·97				
		Mean	15 10 8·00	+0·00	+0·01	−1·18 =	15 10 6·83
						slow	+8·69 s

Next I observed *η Ursæ Majoris* **on June 1 at azimuth 83°4′31″·90:**

Wires.	h m s	Correction. m s		h m s
1	12 30 42·0	+4 46·75	=	12 35 28·75
2	12 31 41·5	+3 49·00	=	30·50
3	12 32 43·1	+2 46·42	=	29·52
4	12 35 30·0	0 0·00	=	30·00
5	12 38 19·8	−2 49·80	=	30·00
6	12 39 30·2	−4 0·65	=	29·55
7	12 40 43·0	−5 13·25	=	29·75
		Mean		12 35 29·72

Same star W. of meridian:

Wires.	h m s	Correction. m s		h m s
7	14 44 13·0	+5 13·29	=	14 49 26·29
6	14 45 26·5	+4 0·65	=	27·15
5	14 46 37·5	+2 49·77	=	27·27
4	14 49 27·0	0 0·00	=	27·00
3	14 52 13·5	−2 46·42	=	27·08
2	14 53 15·5	−3 48·98	=	26·52
1	14 54 14·4	−4 46·77	=	27·63
		Mean		14 49 26·99

β Libræ observed at azimuth 173° 4′ 31″·8:

Wires.	h m s	Correction. s		h m s
1	...	...		...
2	30·4	+34·39	=	4·79
3	39·5	+24·82	=	4·32
4	4·7	0·00	=	4·70
5	29·0	−24·31	=	4·69
6	38·8	−34·15	=	4·65
7	15 10 48·7	−44·03	=	4·67

	D. Ab. s	Level. s	Azim. s		h m s
Mean 15 10 4·64	+0·00	+0·01	−1·68	=	15 10 2·97
				slow	+12·56 s

Next I observed a complete transit of *Polaris* S.P. on June 4, at the same azimuth:

Wires.	h m s	Correction. m s	h m s
7	12 42 6·0	+30 25·4 =	13 12 31·4
6	12 48 55·5	+23 35·6 =	31·1
5	12 55 42·0	+16 47·8 =	29·8
4	13 12 30·0	0 0·0 =	30·0
3	13 29 38·0	−17 8·8 =	29·2
2	13 36 16·0	−23 45·9 =	30·1
1	13 42 36·0	−29 58·4 =	13 12 37·6
		Mean	13 12 31·31

D. Ab.	Level.	Azim.
s. +0·55	s −0·52	s −52·10

= 13ʰ 11ᵐ 39ˢ·24 slow +13ˢ·62

Arcturus, June 4:

Wires.	h m s	Correction. s	h m s
1	58·3	+45·56 =	43·86
2	8·0	+36·12 =	44·12
3	18·0	+26·06 =	44·06
4	44·0	0·00 =	44·00
5	9·6	−25·53 =	44·07
6	19·8	−35·86 =	43·94
7	14 10 30·1	−46·24 =	43·86
		Mean	14 9 43·99

D. Ab.	Level.	Azim.	
s +0·00	s +0·14	s −1·06	= 14ʰ 9ᵐ 43ˢ·07

slow +13ˢ·62

I found the value of azimuth by *Polaris* S.P. and *Arcturus* on June 4 to be −28″·51, and accordingly assumed as the N. point all through

$$173^\circ\ 4'\ 3''\cdot3.$$

I now apply the data to η *Ursæ Majoris*, and find

N. & E.	N. & W.	S. & E.	S. & W.
h m s	h m s	h m s	h m s
12 36 19·65	14 49 1·12	12 35 42·15	14 49 39·53

and remark that any error of azimuth is annihilated in taking them in pairs E. and W., and any error of level annihilated by taking the pairs N. and S.

Accordingly, May 29, we have as the half difference of η *Ursæ Majoris*,

$$1^h\ 6^m\ 20^s\cdot735 = 16^\circ\ 35'\ 11''\cdot01,$$

and the

$$\text{N.P.D. of } \eta \textit{ Ursæ Majoris} = 40^\circ\ 3'\ 24''\cdot42;$$

from which we find as the value of ϕ the colatitude, by the common formula $\tan\phi = \cot\delta \,.\, \cos H$,

$$\phi = 38^\circ\ 52'\ 6''\!\cdot\!65 \text{ by N.};$$

and from June 1 we have in the same way

$$1^h\ 6^m\ 58^s\!\cdot\!69 = 16^\circ\ 44'\ 40''\!\cdot\!04,$$

and the

$$\text{N.P.D. of } \eta \textit{ Ursæ Majoris} \text{ being} = 40^\circ\ 3'\ 23''\!\cdot\!82,$$

$$\phi = 38^\circ\ 50'\ 19''\!\cdot\!69 \text{ by S.}$$

Whence mean of N. and S.

$$\phi = 38^\circ\ 51'\ 13''\!\cdot\!17,$$

a result in which I have little confidence, though I know of nothing but mere shifting of the instrument to affect it.

Note on the Polarisation of Coggia's Comet. By A. Cowper Ranyard, Esq.

On the 1st, 2nd, and 4th of July I examined the light of the comet with a double image prism, but could not with *certainty* detect a difference between the brightness of the two images. On the 6th I was for the first time able to satisfy myself that there were undoubted traces of polarisation: the component in excess being that in the plane passing through the Sun's estimated place below the horizon. The difference of brightness was best observed when the double image prism was turned so that the line joining the nuclei of the two images was at right angles to the axis of the tail, that is, at right angles to the line joining the Sun and Comet: 90° from this position the two images partly overlapped, and their relative intensity was not so easily compared; no traces of bands could be made out with a Savart, nor could I perceive any difference in tint between the two fields of a biquartz when placed in the principal focus of a 4-inch telescope, and examined with a Nicol's prism packed amongst the lenses of an erecting eye-piece.

I continued to examine the polarisation of the comet with a double image prism almost nightly until the 14th, and it appeared to me that the difference in the brightness of the two images continued to become more and more conspicuous. This was specially the case with respect to the relative intensity of the two images of the tail; but I should not like to speak with

certainty of the increase of polarisation of the envelopes in the immediate neighbourhood of the nucleus.

On the 13th I estimated that the brightness of the tail at a distance of three-fourths of a degree from the nucleus in one image was certainly more than double the brightness of the tail at a similar part of the other image. The relative brightness of the two images of the head of the comet might possibly be represented by the numbers 5 and 4; and the contrast between the brightness of the images of different portions of the tail seemed to increase with their distance from the nucleus.

The presence of a continuous spectrum in the tail of the comet precludes the idea of its being composed entirely of incandescent gas; and if it were merely a cloud of fine dust dispersing the Sun's rays, we should expect its light to be strongly polarized. I feel therefore driven to conclude either that the fine dust is incandescent, or that the individual particles, liquid or solid, which go to make up the continuous spectrum part of the tail are large compared with the wave-length of light.

Observations of Coggia's Comet (III. 1874). By E. J. Stone, Esq., M.A., F.R.S., Her Majesty's Astronomer, Cape of Good Hope.

The following observations of Coggia's Comet (III. 1874) were made by me on 1874, July 31. One of the comparison stars on this day was "299 Cape Catalogue 1860," or "Lacaille 3081." The mean places for 1874, January 1, are—

R.A.	N.P.D.
h m s	° ′ ″
7 52 39·06	119 59 50·69.

Cape Mean Times.	Diff. in R.A. Comet−Star.	Diff. in N.P.D. Comet−Star.	R.A. Comet.	N.P.D. Comet.
h m s	m s	′ ″	h m s	° ′ ″
17 10 31·7	+4 13·50	*5 3·98	7 56 51·95	120 4 46·14
17 22 57·5	+4 13·96	6 39·54	7 56 52·41	120 6 21·70
*17 33 52·6	+4 14·04	7 53·71	7 56 52·49	120 7 35·87

The differences in N.P.D. and R.A. have been corrected for refraction, but not for parallax.

The observations were made with the New Equatoreal by Simms.

The comet has been observed on July 29, 30, 31, and August 3, but the positions of the other comparison stars are not known at present.

The comet appeared of about the brightness of a third magnitude star.

* The times for this observation were entered from the clock by Mr. W. H. Finlay.

Spectroscopic and Meridional Observations of Coggia's Comet (III. 1874), made at the Royal Observatory, Greenwich.

(*Communicated by the Astronomer Royal.*)

The following spectroscopic observations of the comet were made by Mr. Christie :—

The comet was examined with a dispersive power of one compound prism (dispersion A to H $7\frac{1}{2}°$) and of two compound prisms, the spectroscope used being adapted to the Great Equatoreal. In the first case a plane reflector was used, to send the rays directly back after passing through the first half prism of the train (which consists of a half prism, four whole prisms, and another half prism silvéred on the back, all compound); in the other case, the half prism silvered on the back was placed to receive the rays after passing through the first half prism. The spectrum of the comet was examined on July 3, 4, 7, 12, 13, and 14, and compared with the spectrum of dioxide of carbon, a vacuum tube containing it, being placed in the telescope two feet from the slit, the plan used by Dr. Huggins in his Stellar and Nebular observations. The spark was obtained from an induction coil, without a Leyden jar, and under these conditions the spectrum was very similar to that of the comet, consisting of three or more bright bands, on a continuous spectrum, which, with a slight change in the spark, became so bright as nearly to obliterate the bands. In the spectrum of the comet two bright bands were found by direct comparison on every occasion to be sensibly co-incident with the two brighter bands of dioxide of carbon; the position of the third band was not determined. All these bands were sharply terminated on the red side, and faded away gradually towards the blue, becoming at the same time shorter, in the direction transverse to the continuous spectrum. On July 7 the coma gave in addition to these bands a faint continuous spectrum, which afterwards seemed to become much brighter, so that the bands were quite obliterated on July 14; but this may have been caused by the twilight and haze as the comet approached the horizon. The spectrum of the nucleus was continuous, but it appeared to have traces of numerous bright bands, and three or four dark lines also were seen on several occasions, but owing to passing clouds they were lost before their positions could be determined. One appeared to lie between D and E, another on the blue side of *b*, and a third near F. On July 14 another strip of continuous spectrum was seen to the east of that of the nucleus, corresponding to the brighter side of the coma.

The nucleus on June 22 and July 6 was planetary, about 5″ in diameter; afterwards it seemed to contract and become nearly stellar.

The tail and coma were found to be partially polarised in a plane through the axis of the tail on July 3, 6, 7, and 13, a doubly

refracting prism being used; the polarisation of the tail appeared to be slightly greater than that of the coma.

The following naked-eye observations of the brightness of the nucleus and length of the tail were made:—

June 22. Nucleus brighter than ✱ 5 mag. Tail about 3° long.
July 3. Nucleus considerably brighter than *o Ursæ Majoris.* Tail about 5°.
July 4. Tail about 7°.
July 5. Tail about 12° long, 2° broad at base, extending to 27 *Ursæ Majoris.*
July 6 and 7. Tail about 15° long.
July 8. Nucleus = *β Ursæ Majoris* and < *α Ursæ Majoris.* Tail 16° or 17°.
July 9. Nucleus > *β Ursæ Majoris* but < *α Ursæ Majoris.*
July 12. Nucleus > *α Ursæ Majoris* or *α Persei*, perhaps = *α Aquilæ.* Tail extended fully up to *o Ursæ Majoris.*
July 13. Nucleus nearly = *Capella* at about same altitude > *α Ursæ Majoris* which was 25° higher, and also > *α Aquilæ.* Tail nearly 20° long.

The comet was also observed on the meridian with the Transit-circle on every available opportunity. The following are the resulting observed right ascensions and north polar distances, corrected for parallax:—

1874		Greenwich M.S.T. h m s	Observed R.A. h m s	Observed N.P.D. ° ′ ″
June	22	13 18 1·4	7 22 12·17	21 51 49·3
	24	13 13 38·7	7 25 41·87	22 17 26·8
July	1	12 56 54·1	7 36 30·40	25 10 29·3
	3	12 51 31·9	7 39 0·51	26 36 18·7
	4	12 48 45·5	7 40 10·14	27 28 4·9
	6	12 42 57·3	7 42 14·14	29 32 41·5
	7	12 39 56·6	7 43 9·47	30 47 35·6
	8	12 36 51·7	7 44 0·68	32 12 36·3
	12	12 23 55·5	7 46 48·56	39 50 15·7

Royal Observatory, Greenwich,
1874, *Sept.* 8.

The Plate given with the present number belongs to Mr. Knobel's paper, "Observations of Jupiter, 1874," No. 8, pp. 403–409.

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